# The American Political Science Review

**Articles and Essays** by Fred W. Grupp, Jr. and Alan R. Richards, Patrick J. McGowan and Robert M. Rood, Mary B. Welfling, Geoffrey Debnam, Peter Bachrach and Morton S. Baratz, Robert T. Holt and John E. Turner

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The Supreme Court and Critical Elections

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Determinants of the Outcomes of Midterm Congressional Elections

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Participation, Coalitions, Vote Trading

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## ARTICLES

**795** **The Supreme Court and Critical Elections.** Several years ago Professor Robert Dahl argued that the traditional concern over the Supreme Court's power of judicial review was largely unfounded. Dahl demonstrated that seldom, if ever, had the Court been successful in blocking the will of a law-making majority. This paper argues that, had Dahl considered his data from a different perspective, he would have discovered that, by virtue of the recruitment process, the Court will rarely even attempt to thwart a law-making majority. Examining Dahl's data in the context of the Survey Research Center's election classification scheme, the paper focuses on the Court's relation to patterns of partisan change to show that the traditional philosophic concern with the counter-majoritarian nature of judicial review is largely divorced from empirical reality and has relevance only during periods of partisan realignment within the political system as a whole. The paper buttresses the argument that the Court's "yea-saying" power is more important than its "nay-saying" power, a realization which can serve as the premise from which a logically consistent justification of the Court's power of judicial review may be dialectically constructed.

By RICHARD FUNSTON, Associate Professor of Political Science, San Diego State University.

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**812** **Determinants of the Outcomes of Midterm Congressional Elections.** An explanatory model for the outcomes of midterm congressional elections is developed. Midterms are a referendum on the performance of the President and his administration's management of the economy. The explanatory model of midterm congressional elections is sufficiently powerful so as to yield honest and accurate pre-election predictions of the national two-party vote in midterm elections. These predictions have usually outperformed pre-election forecasts based on survey data. The model is extended by considering the translation of votes into seats, models of the electorate as a whole and of the individual voter, and the causes of the off-year loss by the President's party.

By EDWARD R. TUFTE, Professor of Public Affairs, Princeton University.

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**827** **Underdevelopment and the "Gap" Theory of International Conflict.** A common hypothesis about the sources of international conflict holds that war and turmoil will be an inevitable consequence of the widening "gap" between the developed and underdeveloped states. This view is based on a common Western image of underdeveloped countries which assumes that striving for economic betterment is universal in all underdeveloped countries, and is primarily a grass-roots phenomenon. This essay challenges the hypothesis and the assumptions upon which it is based. It argues that the images of underdevelopment generated by economists using aggregate data are in many cases incorrect or distorted. Studies by anthropologists which are based on micro- rather than macrodata produce quite different impressions of the underdeveloped society. The human costs involved are for the most part overlooked in development schemes, and the wholesale importation of Western economic development strategies has led in many cases not only to a poor allocation of resources, but also to many of the problems the developed societies are now facing, including urban congestion, rising crime rates, higher incidence of mental breakdown, and the like.

The paper concludes with a critical review of common liberal solutions to development problems, and suggests that one strategy possible for some developing countries is increased isolation from the international system. International conflict may result not only because the underdeveloped states wish to close the "gap," but because some may choose deliberately to reduce their dependency on the West. Conflict may be generated through isolation as well as through increased interaction.

By K. J. HOLSTI, Professor of Political Science, The University of British Columbia.

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**840** **The Primary Goals of Political Parties: A Clarification of Positive Theory.** Positive or rational choice theorists have tended to suppress under the rubric of "winning" elections a critical distinction in the goals of political parties (or candidates)—the distinction between the primary goal of office and the goal of the benefits derived from the control of office. The distinction, however, has strategic consequences. Logically, the office-seeker should follow the vote-maximization strategy put forth by Downs, whereas the benefit-seeker should find Riker's minimal winning coalition most congenial .The distinction in goals and strategies also implies divergent ways of organizing political parties. A concern for benefits logically leads to the development of structures designed to insure that the party's officeholders will deliver the desired benefits. The office-seeking goal implies structures which free the party and the office-seeker to maneuver in response to electoral needs. Thus there are two positive theories resting upon two primary political goals. In their differences we find an explanation of the tensions in democratic parties.

By JOSEPH A. SCHLESINGER, Professor of Political Science, Michigan State University.

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**850** **Variations in Elite Perceptions of American States as Referents for Public Policy Making.** Following Walker, investigators have assumed that state executives frequently seek policy guidance from other states. This study expands the "diffusion of innovation" literature by demonstrating that levels of elite consensus about which American states have the better agencies vary by policy area. It is hypothesized that state administrators in policy areas characterized by general agreement about the better state programs are more influential in their own state's policy-making process than are state administrators in policy areas where there are no acknowledged leaders. Data gathered by mail questionnaire from upper-level state executives in ten American states provide support for the hypothesis. Finally, results from state expenditure studies, which also indicate that different mixes of actors are influential in state policy making depending upon the policy area involved, are found to be consistent with this interpretation.

By Fred W. Grupp, Jr., Research Associate and Executive Director, The Peoples of Connecticut Project, Department of Sociology, University of Connecticut, and Alan R. Richards, Professor of Political Science, Louisiana State University.

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859 **Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe.** This paper is a partial systematic test of Morton A. Kaplan's "theory" of alliance behavior in balance of power international systems first proposed in his well-known *System and Process in International Politics* (1957). Three hypotheses are inferred from Kaplan's writings predicting that in a stable balance of power system, (a) alliances will occur randomly with respect to time; (b) the time intervals between alliances will also be randomly distributed; and (c) a decline in systemic alliance formation rates precedes system changing events, such as general war. We check these hypotheses by applying probability theory, specifically a Poisson model, to the analysis of new data on fifty-five alliances among the five major European powers during the period 1814–1914. Because our research questions are so general, our findings should not be regarded as definitive; however, the data very strongly support our hypotheses. We conclude that Kaplan's verbal model of a balance of power international system has had its credibility enhanced as a result of this paper.

By Patrick J. McGowan, Associate Professor of International Relations, University of Southern California, and Robert M. Rood, Assistant Professor of Government and International Studies, University of South Carolina.

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871 **Models, Measurement and Sources of Error: Civil Conflict in Black Africa.** Cross-national research is plagued by several methodological problems that threaten to distort results and hence raise questions concerning the adequacy of substantive findings. The extent and impact of three of these methodological problems—measurement, sampling, and specification errors—are assessed for a recent model of civil conflict developed by Gurr and Duvall. Concepts in their model are measured with two distinct data sets to estimate measurement error; to assess sampling error, measures are applied to a sample of black African nations which were excluded from their analysis; and new concepts are introduced to assess error in theoretical specification. Although all forms of error are found to exist in their work and to have some distorting effects on their model, it is concluded that some of the general model is accurate. However, discovery of (1) the concepts and data sets that contain the greatest error, and (2) the sources of sampling and specification error, is used to improve substantive findings about the causes of manifest political conflict.

By Mary B. Welfling, Research Associate and Lecturer, Yale University.

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889 **Nondecisions and Power: The Two Faces of Bachrach and Baratz.** The concept "nondecision" was advanced by Bachrach and Baratz as a means of identifying certain areas of community power neglected by reputational and issue analysis approaches. While it is descriptively suggestive of certain possible areas of neglect, it has not been shaped into a useful analytic tool, and does not make any demands which cannot be met by decision-making analysis. Its terms are, in fact, somewhat confused since it fails to differentiate between nondecisions brought about by covert control, and those which may be more generally attributable to a mobilization of bias. These together seem to comprise what Bachrach and Baratz describe as the "second face of power," which has been neglected, they argue, because of inadequate consideration of the meaning of power and related concepts. But their own approach to these does not aid empirical analysis. It simply encourages concern for the minutiae of political action, whereas the simplified view of power adopted here suggests the advantages of a contextual approach.

By Geoffrey Debnam, Senior Lecturer in Political Studies, University of Otago, New Zealand.

**Comment.** By Peter Bachrach, Professor of Political Science, Temple University, and Morton S. Baratz, Vice Chancellor for Academic Affairs, University of Maryland, Baltimore County.

**Rejoinder.** By Geoffrey Debnam.

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908 **Not Voting.** If a person were rational in the sense of maximizing his own utility, it seems unlikely he would ever vote in a mass election. The time and effort expended in voting normally outweigh the infinitesimal chance of influencing the election, making voting seem irrational. But millions do vote. Addressing this paradox, Ferejohn and Fiorina suggested in 1974 that hypothesizing a different form of rational decision making, the "minimax regret" criterion, makes voting rational under rather general conditions. The several contributions arrayed here carry the debate further. Strom reviews the extant arguments and proposes a revised utility-maximization hypothesis. The works by Stephens, Mayer and Good, Beck, and Tullock take up various problems with the minimax regret criterion, including especially the assumption that the voter has no idea about the likelihood of different outcomes for the election. Ferejohn and Fiorina respond to each author and present presidential election data that support the minimax regret hypothesis more than the utility maximization hypothesis. Finally, Goodin and Roberts suggest that because egoistical preferences are weak in the voting decision, voters may be following their usually less visible ethical preferences instead.

**On the Apparent Paradox of Participation: A New Proposal.**

By Gerald S. Strom, Assistant Professor of Political Science, University of Illinois at Chicago Circle.

**The Paradox of Not Voting: Comment.**

By Stephen V. Stephens, Baltimore, Maryland.

**Is Minimax Regret Applicable to Voting Decisions?**

By Lawrence S. Mayer, Research Statistician and Lecturer in Statistics, Princeton University, and I. J. Good, University Professor of Statistics, Virginia Polytechnic Institute and University.

**The Paradox of Minimax Regret.**

By Nathaniel Beck, Assistant Professor of Political Science, Washington State University.

**The Paradox of Not Voting for Oneself.**

By Gordon Tullock, University Professor of Political Science, Virginia Polytechnic Institute and State University.

**Closeness Counts Only in Horseshoes and Dancing.**

By John A. Ferejohn, Associate Professor of Political Science, and Morris P. Fiorina, Associate Professor of Political Science, California Institute of Technology.

**The Ethical Voter.**

By R. E. Goodin, Assistant Professor of Government and Politics, University of Maryland, and K. W. S. Roberts, Research Fellow in Economics, St. John's College, Oxford University.

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929 **The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities.** In an article, "The Paradox of Vote Trading," (*APSR* 67 [December, 1973]) William H. Riker and Steven J. Brams have argued that systematic logrolling among all members of a legislature produces a paradox: While each trade is individually rational, the effects of externalities offset the potential gains from exchanging votes and each voter finds himself worse off than he would have been by voting sincerely. We extend the results of Riker and Brams to a unanimity decision rule and find that a paradox of vote trading holds for that decision rule as well as for simple majority rule. Under a unanimity rule, however, trades which would be collectively rational (i.e., which would produce a Pareto optimal result) are not individually rational; the nontrader is the beneficiary under such a decision rule. Finally, we pose the question Riker and Brams suggested: Is the paradox of vote trading inescapable? Except under very restrictive conditions, we find that it is. However, given certain assumptions about the distributions of individual utilities, we present proofs of the necessary and sufficient conditions for the Pareto optimality of vote trading and argue that in actual legislative situations, when vote trading is Pareto optimal, learning behavior should serve to extricate the members from the paradox of vote trading.

By Eric M. Uslaner, Assistant Professor of Government and Politics, University of Maryland, and J. Ronnie Davis, Professor of Economics, University of Florida.

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943 **The Instability of Minimum Winning Coalitions.** This paper examines William Riker's thesis that only minimum winning coalitions form in n-person zero-sum symmetric games. It demonstrates that Riker's conclusion is false by identifying the conditions under which larger than minimum winning coalitions can form. Since these conditions are quite general it indicates that Riker's conclusion is valid only for a highly restricted class of games. This class of games is identified as those in which players not in a minimum winning coalition have no incentive to form any coalitions among themselves. These games are characterized

as games inessential over coalitions of losers. Only in these games can minimum winning coalitions be expected to form exclusively. In all other games, larger than minimum winning coalitions are possible.

By Norman Frohlich, Associate Professor of Government, University of Texas, Austin.

947 **Evaluation of a Presidential Election Game.** The Shapley value of the presidential election "game" is approximated by the method of multilinear extensions; the likely error in this approximation is computed by studying the error in the electoral college game. A comparison with the Banzhaf ratio is also given. The several biases inherent in the electoral system are analyzed from a mathematical point of view. It is shown that the system incorporates a strong bias in favor of the larger states, and a small bias in favor of the smallest states.

By Guillermo Owen, Professor of Mathematical Sciences, Rice University.

954 **Vote Trading and The Voting Paradox: A Proof of Logical Equivalence.** Riker and Brams have demonstrated the paradox of vote trading (". . . that rational trades by all members [may] make everyone worse off"). In so doing the authors indicate the existence of an apparent disequilibrium when vote trading occurs. I extend this latter point and prove that the preference conditions required for vote trading are the same as those which produce the cyclical majority; the conditions for vote trading and the cyclical majority are logically equivalent. The conclusion briefly indicates the impact of this finding with respect to the work of a number of other authors and gives some idea of the restrictions which would be required to eliminate vote trading among rational legislators.

By David H. Koehler, Associate Professor of Government, American University.

**Comment.** By Peter Bernholz, Professor of Economics, Universität Basel, Switzerland.

**Comment.** By Joe Oppenheimer, Associate Professor of Government, University of Texas, Austin.

**Rejoinder.** By David H. Koehler.

970 COMMUNICATIONS

From Jack A. Goldstone, Marshall Berman, Allan Bloom, Michael C. Stratford, Glenn Tinder

976 EDITORIAL COMMENT

979 BOOK REVIEWS AND ESSAYS

**Crises and Sequences in Collective Theory Development.** Since 1954, the Committee on Comparative Politics has provided leadership in the comparative field, and one of its central objectives has been to construct a theory of political development. The books in the series that were published in the 1960s lacked rigorous design, although they did provide data and low-level generalizations which could be used in the theory-building task. This essay focuses primarily on *Crises and Sequences in Political Development*, which is authored solely by Committee members and reports on the results of their theoretical work thus far. The Committee takes the "intuitive empirical generalization" approach to theory development—in contrast with systematic empirical generalization and the analytic-deductive procedure. It is unlikely, however, that the Committee's approach will lead to the formulation of a coherent set of interrelated propositions within which empirical phenomena can be explained. But the Committee's work is not atypical of the theoretical literature in political science, which reflects the reward structure of the discipline. The building of powerful theories will be facilitated when emphasis is placed on the development of clearly falsifiable propositions rather than on the development of loose conceptual frameworks.

By Robert T. Holt, Professor of Political Science, University of Minnesota, and John E. Turner, Regents Professor of Political Science, University of Minnesota.

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# The Supreme Court and Critical Elections*

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Among judicial agencies throughout the world, past and present, the Supreme Court of the United States stands out as a uniquely powerful political institution. No other court has ever been so continuously, consistently, and intimately involved in the policy-making processes of government. Foremost among the Court's interpretive and thus political powers is its power to nullify an act of Congress by declaring it to be unconstitutional.

This uniquely American institution of judicial review, placing the ultimate power of constitutional policy making in the hands of a small group of unelected individuals, has for decades created a disquietude among American legal and political theorists. These scholars have produced a voluminous body of literature, largely impressionistic and prescriptive, seeking to account for the existence of judicial review and, more importantly, attempting to create a consistent theory of its appropriate scope.[1] While so large a volume of theoretical writing cannot be completely reduced to a single intellectual dichotomy, in very general terms the analysts of judicial review have tended to divide, on normative grounds, into two schools of thought, commonly referred to as judicial activism and judicial self-restraint.

It is important to recognize, however, that at base both the activists and the advocates of restraint share a similar conception of the Supreme Court's function in the American political system: Both see the Court as the protector of minority rights against a majority tyranny. The problem, then, to which both the theorists of judicial activism and the philosophers of judicial self-restraint address themselves is: When should the Court interfere with the popular will? Or, put differently, what constitutes unconstitutional action? The advocates of judicial restraint believe that "unconstitutional" must be defined narrowly.[2] In their view, the Constitution is a vague, imprecise charter which allows great leeway to the dominant political forces. The Court must on occasion move to protect a minority, whether it be an ethnic minority, or a religious minority, or a minority of wealthy capitalists irrationally persecuted by a hostile proletariat, but, for reasons of democratic principle or of political prudence, these instances must be infrequent and well justified. Activists, on the other hand, believe the Constitution to be a more precise document; therefore, they define "unconstitutional" more broadly.[3] As a consequence, they believe that the Court must interfere with the popular will more frequently. Moreover, they are more optimistic about the Court's political strengths than are the advocates of restraint, and so they believe that the Court not only *should* but *can* interfere more frequently. It bears emphasis, however, that both schools believe that the Court's function is to protect minority rights.

### The Dahl-Dooley Hypothesis

Several years ago in a justly famed article Professor Robert Dahl argued that the traditional concern of public lawyers about the Supreme Court's power of judicial review was largely unfounded.[4] The Court cannot and does not, Dahl argued, function to protect minorities. With admirable rigor, he demonstrated that seldom, if ever, had the Court been successful in blocking the will of a law-making majority on an important policy issue.

But Professor Dahl's article is perhaps even more significant for what it implied. Dahl per-

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[1] See, for example, Leonard Levy, ed., *Judicial Review and the Supreme Court: Selected Essays* (New York: Harper & Row, 1967); Alexander Bickel, *The Least Dangerous Branch: The Supreme Court at the Bar of Politics* (Indianapolis: Bobbs-Merrill, 1962), pp. 34–110.

[2] See, for example, Herbert Wechsler, "Toward Neutral Principles of Constitutional Law," *Harvard Law Review,* 73 (November, 1959), 1–35; Learned Hand, *The Bill of Rights* (New York: Atheneum, 1965). See generally Bickel, pp. 34–65.

[3] See, for example, Hugo Black, *A Constitutional Faith* (New York: Knopf, 1968); Laurent B. Frantz, "Is the First Amendment Law? A Reply to Professor Mendelson," *California Law Review,* 51 (October, 1963), 729–754. See generally Bickel, pp. 73–98.

[4] Robert Dahl, "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," *Journal of Public Law,* 6 (Fall, 1957), 279–295.

After the final draft of this manuscript was virtually completed, an additional commentary bearing on its thesis became available. David Adamany, "Legitimacy, Realigning Elections, and the Supreme Court," *Wisconsin Law Review* (September, 1973), pp. 790–846. Professor Adamany's article is a thoroughgoing, conscientious—if ultimately wrong-headed—critique of Dahl's work on the Court, as well as that of professors Charles Black and Alexander Bickel.

suasively demonstrated that the Court could not long block the desires of a dominant political coalition, but he merely suggested that the Court would not often *wish* to block the majority will. National politics in the United States, Dahl tells us, has been dominated by relatively cohesive alliances that endure over long periods of time.[5] Because the Court is a political institution whose members are recruited with their political preferences and prejudices in mind, it is inevitably part of the dominant political alliance, except for transitional periods during which the old alliance is crumbling and a new one rising to take its place. During these periods, presumably, the Court, by virtue of the life tenure of the Justices, will be out of step with the political times. Professor Dahl's tabular data, however, while persuasive, do not support his conclusion on this point. Indeed, they are completely unrelated to it. Rather, the conclusion was apparently arrived at impressionistically on the basis of logical deduction. When the Court strikes down congressional legislation as unconstitutional, Dahl demonstrated, it is seldom successful in the long run in thwarting the majority policy. *But the crucial point is that most of the time the Court will not be striking down national legislation.* Dahl did not examine this point at length, nor, therefore, did he consider when the Court would be most likely to act in a counter-majoritarian fashion.[6]

In light of the tremendous expenditures of time and intellectual effort scholars have made in consideration of the problems presented by the institution of judicial review, Professor Dahl's suggestions are relatively iconoclastic and, thus, worthy of more serious examination. I propose, therefore, to do what Dahl did not—that is, to test the following hypothesis: Over long periods of time, the Supreme Court reflects the will of the dominant political forces;[7] however, during transitional periods, in which the Court is a holdover from the old coalition, the Court will be more likely to perform the counter-majoritarian functions ascribed to it by traditional theory. The hypothesis, in other words, is that, as Mr. Dooley so cryptically put it, "the Supreme Court follows the election returns."

In order to examine the Dahl-Dooley hypothesis, however, some sort of indirect test is clearly necessitated. Essentially the problem we are addressing is: When are Supreme Court decisions most likely to conflict with the will of a national majority? As Dahl himself noted, in any strict sense, there is simply no way of establishing with any high degree of confidence whether a given policy alternative was or was not supported by a majority of Americans.[8] But let us assume, as did Dahl, that one may equate the "law-making majority" with the "national majority."[9] In other words, granting that the connection between one's vote and one's policy preferences is perhaps tenuous, let us, nevertheless, also grant that however tenuous there is at least *some* relation between a voter's preference among candidates or parties and his preferences among alternative public policies.[10] Indeed, recent scholarship suggests that past conclusions about the lack of ideological awareness among the American electorate may have been unique to the Eisenhower era.[11] While the importance of issues in deciding elections will undoubtedly change from one election to another, there is evidence that even in a campaign not notable for its issue saliency most people were concerned with a number of specific issues and that these issue concerns had a measurable effect upon their voting choices.[12]

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[5] Dahl, p. 293.

[6] Dahl, pp. 293–295.

[7] Although for purposes of comparability with Dahl I have chosen to equate a "law-making majority" with a "popular majority," this point is not critical to the argument. The coalition dominant within the American political process at any given period may be dominant because of its numbers, or because of its wealth, or because of its monopolization of the media or the instruments of force, or for some other reason. The argument here is that the Court will reflect the values of this dominant coalition. The reasons why that coalition is politcally dominant are irrelevant to the hypothesis. Thus, the argument is equally applicable whether one employs a "pluralist" or an "elitist" concept of American politics. See Robert Dahl, *Pluralist Democracy in the United States: Conflict and Consent* (Chicago: Rand McNally, 1967). But see William Connolly, ed., *The Bias of Pluralism* (New York: Atherton, 1969); Peter Bachrach, *The Theory of Democratic Elitism: A Critique* (Boston: Little, Brown, and Co., 1967).

[8] Dahl, "Decision-Making," pp. 283–284.

[9] Dahl, "Decision-Making," p. 284. See also note 7. While it may never be possible rigorously and definitively to resolve the question of whether a legislature *really* represents a "majority," especially given the evanescence of majorities and the difficulties involved in defining "representation," such evidence as we have suggests that Congress is not markedly out of line with its constituents. See, for example, Warren Miller and Donald Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March, 1963), 45–56.

[10] See V. O. Key, *The Responsible Electorate* (Cambridge, Mass.: Belknap Press, 1966). See also Richard Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), 429–449.

[11] Gerald Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review*, 66 (June, 1972), 415–428. But see Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970).

[12] David RePass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400. But see Nelson Polsby and Aaron Wildavsky, *Presidential Elections: Strategies of American Electoral Politics*, 2nd edition (New York: Scribner, 1971), pp. 293–302.

Some, however, will object to equating the preferences of the "law-making majority" with the will of the "national majority" on the grounds that a large segment of the American public does not vote.[13] It will be argued, therefore, that while the elected representatives may quite accurately reflect the will of a majority of the electorate, an "electoral majority" and a "national majority" are two quite separate and distinct entities. This objection, however, if valid, is completely valid only for recent American political history and will not hold as we move back in time. American voter turnout in the nineteenth century was quite impressive.[14] Indeed, some citizens of the frontier were known to be so moved by a sense of civic duty as to exercise their franchise more than once or even from the grave.

If then we may, on this rather tenuous foundation, take a "law-making majority" to be for operational purposes the equivalent of a "national majority," it is possible to test the hypothesis. If we take the preferences of the law-making majority to represent the will of the national majority, and if we take as our operational definition of the preferences of the law-making majority federal legislation passed by majorities in the House and Senate and signed by the president, then we may conclude that the Supreme Court is out of line with the majority will when it holds congressional statutes unconstitutional.[15] We may then examine over time the behavior of the Court in exercising its power to declare congressional statutes unconstitutional in order to determine if this behavior occurs randomly, as the traditional conception of the Court's function would suggest, or if it tends to occur at given, identifiable intervals, as Dahl's thesis implies.[16]

## Electoral Realignment and the Party System

To examine the hypothesis that the Supreme Court follows the election returns, the first thing to which we should turn are the election returns. Were American national elections, especially presidential elections, mere popularity contests, there would be a considerable degree of electoral flux or instability in American political history. If a party's victory in a presidential election depended upon nothing more than the personal attractiveness of its candidate, each party would win in random fashion, unless one wishes to assume that one party would have a monopoly on all of the attractive candidates. Examining American political history, however, one does not find that such behavior has been the case. Instead, as Dahl pointed out, Americans' electoral behavior has been quite stable, with one party or another dominating national politics for long periods of time. How to explain such behavior?

The Survey Research Center, of course, explains it by emphasizing the influence of party identity upon a voter's choice between presidential candidates.[17] Elections, according to this view, are essentially reaffirmations of party allegiance. The outcome of any given election may be seen as the result of short-term forces acting upon the underlying distribution of party loyalties within the electorate. The study of electoral behavior, then, becomes the study of the flow of partisan allegiances rather than the study of single elections.

To facilitate such study, the S.R.C. has, of course, formulated an electoral classification scheme characterizing each presidential election as (1) maintaining, (2) deviating, or (3) realigning, depending upon whether the movement of the vote results in the election of the candidate of the majority or minority party and upon whether the movement of the vote is associated with transient, short-term forces or with a basic shift in long-term partisan loyalties.[18] In a maintaining election, the current pattern of party differences is maintained, and the candidate of the majority party is elected. In a deviating election, the net short-term forces are both to the advantage of the minority party and strong enough to influence the election of its presidential candidate, but the electorate's underlying pattern or distribution of partisan loyalties is unaffected. In a realigning election, however, popular political feeling is so intense that the basic underlying patterns of the electorate's party identifications are changed, and a new party balance is created which endures over several decades.[19] Such fundamental shifts in national political alignments, usually associated with national crises, are not, of course, actually accomplished in a single election. One, instead, must speak of realignment phases or of a realigning electoral era.[20] For example, the partisan realignment which produced New Deal Democracy was begun by Al Smith in 1928, when he changed the image of the Democratic party from that which William Jennings Bryan had given it and captured small pluralities in the populous, Catholic, urban Northeast; it was completed by Roosevelt's sweep in 1936.[21] Naturally enough, there have been very few realigning phases in American political history.[22]

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[13] But see William Andrews, "American Voting Participation," *Western Political Quarterly,* 19 (December, 1966), 639–652.

[14] Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review,* 59 (March, 1965), 7–28.

[15] The existence of the institution of federalism in America introduces certain problems for this analysis as indeed it does for a great many other analyses of American politics. I have chosen to limit the analysis to cases involving federal statutes on the grounds that it is logically impossible to conclude anything about the relationship between the Supreme Court and "majority" rule on the basis of cases involving state statutes.

[16] But see note 67. The limited nature of the data will permit of only very modest conclusions.

[17] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: Wiley, 1966).

[18] Campbell et al., pp. 63–77. The seminal work on cyclical electoral patterns was, of course, V. O. Key, "A Theory of Critical Elections," *Journal of Politics,* 17 (February, 1955), 3–18. For a most sophisticated attempt to synthesize the work on political party systems, explaining election cycles in general theoretical terms, see Thomas P. Jahnige, "Critical Elections and Social Change: Towards a Dynamic Explanation of National Party Competition in the United States," *Polity,* 3 (June, 1971), 465–500.

It bears emphasis that, given the still very crude state of the art, the Survey Research Center's classification scheme is applicable only to presidential elections. If and when it can be refined and extended to other elections, e.g., congressional; it will much further advance the study of the relationship between social change and institutional response.

But what has this to do with the Supreme Court? Simply this—this conceptual framework allows us to gauge voter preference over time, and, it will be recalled, we are using voter preference as an indicator of the majority will. If we construct a simple, chronological chart of the ebb and flow of electoral patterns and compare it with the behavior of the Court in exercising its power of judicial review, we shall then be able to test the Dahl-Dooley thesis that the Court is never long out of line with the dominant political coalition, except during transitional periods, which we shall identify as realignment phases.

First, however, an intermediate step may be taken. It may be useful to determine for whom and for what the electorate voted, since our operational definition of the majority will is not simply the electoral preference but rather the action of the law-making majority. Fortunately, for this purpose, Professors William Nisbet Chambers and Walter Dean Burnham have already applied the S.R.C. classification scheme to a study of the development of American political parties.[23] Chambers and Burnham's study of the history of American voting alignments reveals that over time American party politics has experienced five major patterns of party competition or, as Chambers and Burnham call them, five national party systems.[24]

The first national party system identified by Chambers and Burnham existed from 1789 until 1820. This really was a transitional phase between the establishment of the American State and the acceptance of and regularization of its political patterns.[25] Because those roles or functions which political parties could or should play were not yet well defined, understood, or accepted, the party system which developed in the 1790s exhibited certain peculiarities which set it off from all succeeding party systems. The parties were much more centrally organized than today; and, because the idea of a political opposition as legitimate was not yet firmly entrenched, each of the opposing political coalitions viewed the other as subversive, resulting in an abnormally intense ideological behavior not usually characteristic of American party politics.

Chambers and Burnham, like most contemporary American historians, discount the importance of Jefferson's election in 1800. It certainly did not constitute a realignment with any significance for national policy making. Despite Jefferson's liberal, laissez-faire statements, there was no appreciable change in national economic policy; neomercantilism remained the order of the day. Likewise, the Republican leadership appears to have been hardly less elitist than that of the Federalists.[26] These similarities, coupled with the fact that national elections remained largely undemocratized, may account for the collapse of the first party system. By 1820, when Monroe ran unopposed for the presidency, there evidently were insufficient points of political conflict to sustain a system of national, multiparty competition.

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[19] See also Gerald Pomper, "Classification of Presidential Elections," *Journal of Politics*, 29 (August, 1967), 535–566, distinguishing between realigning and converting elections depending upon whether the dominant party of the previous era continues its dominant status.

[20] See James L. Sundquist, "Whither the American Party System?", *Political Science Quarterly*, 88 (December, 1973), 559–581.

[21] See Ruth Silva, *Rum, Religion, and Votes: 1928 Re-Examined* (University Park: Pennsylvania State University Press, 1962); Samuel Eldersveld, "The Influence of Metropolitan Party Pluralities in Presidential Elections since 1920: A Study of Twelve Key Cities," *American Political Science Review*, 43 (December, 1949), 1189–1206.

[22] Depending upon whose count one uses, there have been three, four, or six. Compare Campbell et al., *Elections and the Political Order* with William Nisbet Chambers and Walter Dean Burnham, eds., *The American Party Systems: States of Political Development* (New York: Oxford, 1967); and James L. Sundquist, *Dynamics of the Party System: Alignment and Realignment of Political Parties in the United States* (Washington, D.C.: Brookings, 1973); and Charles Sellers, "The Equilibrium Cycle in Two-Party Politics," *Public Opinion Quarterly*, 29 (Spring, 1965), 16–37.

[23] Chambers and Burnham, *American Party Systems;* see also Sundquist, *Dynamics of the Party System.* But see Douglas Price, " 'Critical Elections' and Party History: A Critical View," *Polity,* 4 (December, 1971), 236–242.

[24] The following brief summary of the findings reported in *American Party Systems* relies heavily upon Professor Burnham's concluding essay in that volume. For a more extensive treatment, the reader is urged to consult Burnham, "Party Systems and the Political Process," in *American Party Systems,* pp. 277–307. See also Arthur Schlesinger, Jr., ed., *The Coming to Power: Critical Presidential Elections in American History* (New York: Chelsea House, 1971). The general patterns identified by Chambers and Burnham are essentially the same as those advanced by Jahnige, "Critical Elections and Social Change," p. 468.

[25] See Paul Goodman, "The First American Party System," in *American Party Systems,* pp. 56–89. See also Joseph Charles, *The Origins of the American Party System* (New York: Harper & Row, 1961); William Nisbet Chambers, *Political Parties in a New Nation: The American Experience, 1776–1809* (New York: Oxford, 1963).

The emergence of recognizably modern parties occurred during the second American party system, following a partisan realignment in response to the election of John Quincy Adams.[27] The growing popular demand for democratization of the franchise coincided with popular revulsion at the "deal" by which Adams was elected from within the House of Representatives and coalesced around the charismatic figure of Andrew Jackson. The resulting system of partisan competition contributed to the decentralization of political power in America. The period saw the rise of the convention and the decline of the centralized congressional caucus as a mechanism for the selection of presidential candidates. Under Jackson and his successors, the federal government progressively declined in power vis-à-vis the states, eventually completely withdrawing from intervention in the economic sector. In part this was due, of course, to the rise of slavery as a political issue.[28] The pursuit of any active federal economic policy necessarily exacerbated the emerging sectional conflict. As a corollary, the presidency experienced a decline in prestige at the expense of the locally-elected Congress. A further consequence of this democratization and decentralization of the political process, however, was the pragmatic, nonprogrammatic character of both major parties. Because the two national parties lacked consistent, coherent principles the second American party system was extremely unstable. Both the Whigs and the Democrats were motley collections of contradictory elements which were incapable of reconciling within themselves the divisive influence of the sectional conflict over slavery.[29]

For this reason, an entirely sectional party emerged. The central reality of the realignment which produced the Republican party was the restructuring of all political relationships along sectional lines. Were the cancer of slavery to be contained in order that it might be removed from the body politic, this was the only institutional arrangement possible. By the 1850s, a textbook example of a prerevolutionary situation had developed in the United States.[30] New elites, representing the urban, industrial Northeast, were challenging the hegemony of the old, southern, agrarian elite which had dominated national policy making under the Jacksonian system. Were the Union to survive, entirely new ways of doing things had to be adopted, created, or invented. Certainly the period immediately following the Civil War was one of great ferment, experimentation, and change. Not only was slavery abolished but also, for a time, attempts were made to involve the national government in a program of elevating the freedman to a position of first-class citizenship, while simultaneously the federal government initiated a series of measures designed to foster economic expansion.[31]

By the 1870s, however, the radical phase of the third party system had collapsed. This collapse was ratified in the infamous bargain of 1876 by which, in exchange for the White House, the Republican party abandoned its efforts to create a viable Republican structure in the South. For the next twenty years, the division of partisan competition was so close that national political life was virtually stalemated. At the same time, political institutions generally were declining in significance relative to, or were being overshadowed by, the changes being wrought in the society at large by the corporate-industrial revolution.[32]

These changes were, in turn, rendering the arrangements of the third party system obsolete.

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[23] See Leonard Levy, *Jefferson and Civil Liberties: The Darker Side* (Cambridge, Mass.: Harvard University Press, 1963); Walter Berns, "Freedom of the Press and the Alien and Sedition Laws: A Reappraisal," *The Supreme Court Review* (1970), pp. 109–160.

[27] See Richard McCormick, "Political Development and the Second Party System," in Chambers & Burnham, *American Party Systems,* pp. 90–116.

[28] Burnham, "Party Systems and the Political Process," pp. 292–295. See also Dwight L. Dumond, *Antislavery Origins of the Civil War in the United States* (Ann Arbor, Mich.: University of Michigan Press, 1959); Roy F. Nichols, *The Stakes of Power, 1845–1877* (New York: Hill and Wang, 1961).

[29] Roy F. Nichols, *The Disruption of American Democracy* (New York: Free Press, 1967).

[30] Burnham, "Party Systems and the Political Process," pp. 295–296; Charles A. Beard and Mary Beard, *The Rise of American Civilization* (New York: Macmillan, 1927), pp. 52–122.

[31] See Kenneth Stampp, *The Era of Reconstruction, 1865–1877* (New York: Alfred A. Knopf, 1965); James G. Randall and David Donald, *The Civil War and Reconstruction* (Boston: Heath, 1961).

[32] See Robert McCloskey, *American Conservatism in the Age of Enterprise, 1865–1910* (Cambridge, Mass.: Harvard University Press, 1951); Richard Hofstadter, *The American Political Tradition and the Men Who Made It* (New York: Random House, 1948), pp. 164–205; Sidney Fine, *Laissez Faire and the General-Welfare State: A Study of Conflict in American Thought, 1865–1901* (Ann Arbor, Mich.: University of Michigan Press, 1956).

The old politics was structured in such a way that it was incapable of taking into account certain demographic and economic changes of major importance. The result was to exclude by definition certain groups from participation in the political process. In particular, powerlessness was the lot of the western farmers and a growing, urban, immigrant proletariat.[33] Rationally, these two groups should have joined forces against the conservative, industrial elite which had come to dominate both major parties within the third party system. This did not happen, however. For complex reasons of structural weakness, one of the excluded groups, the western farmers, managed to capture one of the two major parties, the Democratic, without the assistance of the immigrants in the Northeast. But the populism of William Jennings Bryan not only was based upon opposition to industrial capital but also contained an element of nativist hostility toward the "new immigration."[34] The immigrants responded by shunning a Democratic party openly prejudiced against them. Moreover, in 1893 the country's worst economic depression to that date occurred while a Democrat was President. The response of the industrial workers was the same as it was to be in 1932, an electoral abhorrence of the party in power. The consequent political realignment produced a system of noncompetition, nonparticipation, one-party states, one-party sections, and Republican dominance which lasted until the late 1920s.[35]

The realignment that produced the winning coalition of rural and urban underprivileged, labor, ethnic minorities, and the academic elite which we associate with Franklin Roosevelt actually was presaged by the Smith campaign of 1928.[36] The "Happy Warrior" was able to wrest control of the Democratic party from the southern-rural-colonial forces which had dominated it; his campaign attracted a huge bloc of new immigrant voters into the political system, precipitating a realignment in the urban Northeast. The stable sectionalism of the fourth party system was then entirely shattered a year later by the force of the Great Depression.[37]

The consequences of this last realignment in American politics are familiar to all. A bureaucratized, welfare-warfare state became the major reality of American politics. The Democratic party replaced the GOP as the dominant party in a system of partisan competition based upon appeals to economic self-interest and group identification.[38] A mixed system of welfare capitalism has emerged, within which the federal government is actively involved in the creation, promotion, and perpetuation of countervailing sources of power, and the executive branch has moved into the ascendancy as the center of policy planning and initiation.

Some analysts have speculated that we are experiencing yet another realignment.[39] But, although some evidence has been adduced which would support that thesis,[40] the S.R.C. electoral classification scheme is not a predictive device; it is not possible, therefore, to say with any certainty whether we are or are not in a realigning phase, let alône what the consequences of such a realignment might be.

## Periodicity in Supreme Court Behavior

The fact that the S.R.C. electoral classification scheme can be validly and profitably applied to a study of the development of American party politics raises the possibility that it might with equal profit be applied to a discussion of the development of American constitutional politics. Interestingly enough, many students of the Court have remarked that its history has been delimited by five fairly specific periods.[41] But attempts to employ the voting behavior research in order to correlate the Court's behavior over time with the dynamics of the system at large have been few and inchoate at best,[42] even though such a correlation would provide the "most obviously plausible example of synchronization of institutional-role and policy-output change with critical realignment" in American politics.[43] In each of these five periods of the Court's history, certain characteristic doctrines have prevailed, only to be abandoned in a later period. The Court seems always to have some special interest to protect or value to advance. Have these interests or primary legal norms been opposed to or consonant with the primary values expressed within the larger political system?

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[33] See Harold U. Faulkner, *Politics, Reform, and Expansion* (New York: Harper & Row, 1959); John Hicks, *The Populist Revolt* (Minneapolis: University of Minnesota Press, 1931).

[34] See John Higham, *Strangers in the Land: Patterns of American Nativism, 1860–1925* (New York: Atheneum, 1970).

[35] Burnham suggests that this result was accomplished by the withdrawal from political participation of the urban working class, creating a political void which was filled elsewhere by the rise of socialist parties. Burnham, "Party Systems and the Political Process," p. 301. But see Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace & World, 1955).

[36] See Silva, *Rum, Religion, and Votes.*

[37] See William Leuchtenberg, *Franklin D. Roosevelt and the New Deal* (New York: Harper & Row, 1963).

[38] See Theodore Lowi, *The End of Liberalism* (New York: Norton, 1969).

[39] Richard Scammon and Benjamin Wattenberg, *The Real Majority* (New York: Coward, McCann & Geoghegan, 1970); Kevin Phillips, *The Emerging Republican Majority* (New Rochelle, New York: Arlington House, 1969).

[40] See Samuel Lubell, *The Hidden Crisis in American Politics* (New York: W. W. Norton & Co., 1970); Herbert Weisberg and Jerrold Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review*, 64 (December, 1970), 1167–1185. But see Sundquist, "Whither the American Party System?," and *Dynamics of the Party System.* Rather than a realignment, Sundquist anticipates a reversal of the current trend toward party disintegration and a new strengthening of party attachments along the lines of cleavage established in the New Deal era. As such, Sundquist seems to have in mind what Pomper would call a "converting" era. See Pomper, "Classification of Presidential Elections."

[41] See, for example, Leonard Levy, ed., *American Constitutional Law: Historical Essays* (New York: Harper & Row, 1966), pp. 1–9; Robert Scigliano,

The first and formative period of the Supreme Court's history is identified with the Chief Justiceship of John Marshall. Under Marshall, the primary legal norms advanced by the Court were national supremacy,[44] judicial power,[45] and the protection of private property.[46] During Marshall's tenure, the Court labored to establish the supremacy of the national government over the states through broad interpretations of such constitutional provisions as the commerce clause and the necessary and proper clause.[47] Similarly, it asserted its own power to review the validity of state legislative and judicial actions, thereby increasing not only the power of the federal government generally but also the power of the federal judiciary in particular.[48] Nationalism also proved to be a useful tool for the protection of vested rights. Since most of the statutes which interfered with private property during this era were state laws, interpretations that severely limited states' rights were frequently employed to invalidate such statutes.[49]

There would indeed seem to be a certain coincidence between the values asserted or advanced by the Court under Marshall and the dominant norms of the first American party system identified by Chambers and Burnham. That party system was characterized by the centrality of its organization, and, although more moderate in their elitism than the Federalists, the Republicans were nonetheless committed to the protection of aristocratic privilege, a part of which involved the protection of property rights. Thus, the harmony which developed between Marshall's Supreme Court and Jefferson's successors may have been less than accidental.[50]

Perhaps not surprisingly, Marshall's successor was none other than Andrew Jackson's right-hand man, Roger Brooke Taney. Just as Jackson and his successors dismantled the neomercantilist system and withdrew the federal government from the economic sector, so the Taney Court was less inclined to invalidate state legislation and viewed corporate enterprise with a less hospitable eye

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*The Supreme Court and the Presidency* (New York: Free Press, 1971), p. viii. See also Glendon Schubert, *Judicial Policy-Making* (Glenview, Illinois: Scott, Foresman, 1965), pp. 131–157.

[42] Adamany, "Legitimacy, Realigning Elections, and the Supreme Court," is really more suggestive than it is rigorously empirical.

Sheldon Goldman and Thomas P. Jahnige have briefly examined the coincidence between periods of electoral realignment and the Court-curbing periods identified by Stuart S. Nagel. They have found a distinct correlation. But such a focus is, by definition, concerned not with the Court's behavior but with the actions of others directed at the Court. Sheldon Goldman and Thomas P. Jahnige, *The Federal Courts as a Political System* (New York: Harper & Row, 1971), pp. 261–268; see also Stuart S. Nagel, *The Legal Process from a Behavioral Perspective* (Homewood, Ill.: Dorsey Press, 1969), pp. 260–279.

Nagel and Wallace Mendelson have discussed the effect for Supreme Court policy making of party differences between Congress and the Court, but their work has not been guided by critical election theory. Nagel, *The Legal Process*, pp. 245–259; Mendelson, "Judicial Review and Party Politics," *Vanderbilt Law Review*, 12 (March, 1959), 447–457.

[43] Burnham, *Critical Elections*, p. 10, fn. 15.

[44] See, for example, Fletcher v. Peck, 6 Cranch 87 (1810); McCulloch v. Maryland, 4 Wheaton 316 (1819).

[45] See, for example, Marbury v. Madison, 1 Cranch 137 (1803); Cohens v. Virginia, 6 Wheaton 264 (1821).

[46] See, for example, Dartmouth College v. Woodward, 4 Wheaton 518 (1819); Sturges v. Crowninshield, 4 Wheaton 122 (1819).

[47] See, for example, Gibbons v. Ogden, 9 Wheaton 1 (1824); Brown v. Maryland, 12 Wheaton 419 (1827); McCulloch v. Maryland, 4 Wheaton 316 (1819).

[48] See, *for example*, Fletcher v. Peck, *6 Cranch* 87 (810); Martin v. Hunter's Lessee, 1 Wheaton 304 (1816); Cohens v. Virginia, 6 Wheaton 264 (1821).

[49] See, for example, Fletcher v. Peck, 6 Cranch 87 (1810); Dartmouth College v. Woodward, 4 Wheaton 518 (1819); Sturges v. Crowninshield, 4 Wheaton 122 (1819).

[50] Differences of personality and temperament as much as differences of politics may account for the enmity which existed between the Court headed by Marshall and the executive branch headed by Jefferson. With the removal of Jefferson from the White House and thus the removal of the personality factor, the Marshall Court and the succeeding Republican presidents got along quite well. See Richard Ellis, *The Jeffersonian Crisis: Courts and Politics in the Young Republic* (New York: Oxford, 1971); Alfred Kelly and Winfred Harbison, *The American Constitution: Its Origin and Development* (New York: Norton, 1970), pp. 202–247; Julian Boyd, "The Chasm That Separated Thomas Jefferson and John Marshall," in *Essays on the American Constitution*, ed. Gottfried Dietze (Englewood Cliffs, New Jersey: Prentice-Hall, 1964), pp. 3–20.

than had Marshall.[51] During this period, the federal government declined in power vis-à-vis the states, and the presidency experienced a precipitate drop in prestige. Although the Court under Taney did not suffer an equivalent decline, it did behave similarly in that it withdrew to a position somewhat more like an impartial arbiter between states and nation, rather than functioning, like the Marshall Court, as an active champion for the central government. Finally, just as the second party system was characterized by an extreme sectionalism of voting patterns, so too the Court under Taney increasingly came to divide along sectional lines in its disposition of cases, with the southerners, as in the other two branches, holding a slim but stable majority.[52]

The infamous *Dred Scott* decision and its subsequent repudiation upon the battlefield brought about a decline in the Court's prestige which lasted throughout the decade of the 1860s. As a result, the Supreme Court did not participate in the radical phase of the third party system. The program which was initially followed by the Republican victors of the realignment of 1852–1860 was twofold: on the one hand, a policy of federal protection of civil rights, especially those of the newly freed blacks; on the other, a policy of federal intervention in the economy in order to encourage the development of commercial and corporate enterprise. By 1870, the first half of this program had been abandoned. This abandonment of the black man by the Republican leadership was formalized in the bargain of 1876 which produced a sectional, partisan deadlock that lasted for the next decade and a half. This solution came to be tacitly accepted by public opinion, and in the 1880s was recognized as legitimate by the Supreme Court in a series of decisions which emasculated most of the comprehensive radical program for the protection of the Negro.[53] On the other hand, the Court, having regained its strength and prestige, did its part in advancing the second half of the Republican program, federal encouragement of industrial capitalism.[54] Not only did the Court thus validate the practices of the dominant political coalition but also it laid the groundwork for new doctrines of constitutional limitation which might be employed to protect corporate property if the dominant political values were to change.

If anything, the Supreme Court *anticipated* the electoral realignment which brought about the fourth party system. By 1893, as already noted, the stage was set for a political coalition between the western farmers and the immigrant laborers. Such a coalition, of course, did not occur. But, before it was clear that this would be the case, while the industrial elite still feared such a coalition might prevail, the Supreme Court in a series of classic decisions in 1894–95 did what it could to undermine the legitimacy of a massive assault on established elite rule.[55] When it became clear, however, that the feared coalition would not emerge and that the result of the realignment of 1888–1896 was to elevate McKinley Republicanism to a position of preeminence in the political system, the Court began a campaign completely in consonance with the values represented by the dominant party: laissez-faire at home and imperial dominance abroad. With respect to the latter, the Court, in a series of decisions at the turn of the century, validated America's rise to the position of imperial power, holding in essence that the Constitution allowed Americans to "take up the white man's burden."[56] As for the domestic principle of laissez-faire economics, this period saw the Court employ the due process clause of the Fourteenth Amendment as a formidable weapon against an assortment of state legislation that sought to protect consumers, unions, farmers, unorganized labor, women, and children against the abuses of business enterprise.[57] The high point of the Court's attempts to control public policy was, of course, reached during seventeen months in 1935–36, when the Court launched a frontal attack upon the New Deal.[58] Public opinion, though opposed to "packing" the Court, was outraged, and the Court beat a strategic, if belated, retreat, abandoning its role as economic policy maker.[59]

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[51] See, for example, Mayor of New York v. Miln, 11 Peters 102 (1837); Charles River Bridge Co. v. Warren Bridge Co., 11 Peters 420 (1837); Bank of Augusta v Earle, 13 Peters 519 (1839).

[52] See, for example, Dred Scott v. Sandford 19 Howard 393 (1857).

[53] For example, Civil Rights Cases, 109 U. S. 3 (1883).

[54] Knox v. Lee, 12 Wallace 457 (1871); Chicago, Milwaukee & St. Paul Ry. Co. v. Minnesota, 134 U. S. 418 (1890).

[55] For example, United States v. E. C. Knight Co., 156 U. S. 1 (1895); Pollock v. Farmers' Loan & Trust Co., 157 U. S. 429 (1895); Pollock v. Farmers' Loan & Trust Co., 158 U. S. 601 (1895). See Alan Westin, "The Supreme Court, The Populist Movement and The Campaign of 1896," *Journal of Politics,* 15 (February, 1953), 3–41.

[56] For example, DeLima v. Bidwell, 182 U. S. 1 (1901); Downes v. Bidwell, 182 U. S. 244 (1901); Dorr v. United States, 195 U. S. 138 (1904); Rasmussen v. United States, 197 U. S. 516 (1905).

[57] For example, Smyth v. Ames, 169 U. S. 466 (1898); Lochner v. New York, 198 U. S. 45 (1905); Coppage v. Kansas, 236 U. S. 1 (1915).

[58] For example, Schechter Bros. Poultry Corp. v. United States, 295 U. S. 495 (1935); United States v. Butler, 297 U. S. 1 (1936); Carter v. Carter Coal Co., 298 U. S. 238 (1936).

[59] See Robert Stern, "The Problems of Yesteryear—Commerce and Due Process," *Vanderbilt Law Re-*

**Table 1. Relations Between Electoral Realignment, American Party Systems, and Supreme Court Decision Making**

| Date | Party System | Judiciary (by Chief Justice) | Exemplary Cases |
|---|---|---|---|
| 1800 | | | |
| | Jeffersonian Republican | Marshall | |
| 1820 | | | |
| Realignment phase | | | |
| 1828 | | | |
| | Jacksonian Democracy | Taney | Worcester v. Georgia 6 Peters 515 (1832) |
| 1852 | | | |
| Realignment phase | | | Dred Scott v. Sandford, 19 Howard 393 (1857) |
| 1860 | | | |
| | Radical Republican | Chase and Waite | |
| 1888 | | | |
| Realignment phase | | | Pollock v. Farmers' Loan & Trust Co., 157 U.S. 429 (1895) |
| 1896 | | | |
| | McKinley Republican | Fuller, White, Taft, and Hughes | |
| 1928 | | | |
| Realignment phase | | | Schechter v. U.S., 295 U.S. 495 (1935), U.S. v. Butler, 297 U.S. 1 (1936) |
| 1936 | | | |
| | New Deal Democracy | Stone, Vinson, and Warren | [Lamont v. Postmaster General, 381 U.S. 307 (1965); Shapiro v. Thompson, 394 U.S. 618 (1969)] |
| ???? | | | |

In the most recent period of the Court's history, constitutional law has been brought into accord with the norms of the New Deal Democrats.[60] The precedents of economic due process have been scuttled, and the authority of the government to intervene in the economy is virtually undisputed. From the New Deal to the Great Society, the Court has sustained the welfare state. At the same time, the Court has liberalized the constitutional law of civil liberties and, in particular, has advanced on many fronts the value of egalitarianism, the doctrine of the "common man."

On the basis of this cursory analysis, it would certainly seem that the Supreme Court does follow the election returns over time. Table 1, moreover, suggests that some of the Court's most notable collisions with the elected branches have indeed occurred during periods of partisan realignment.[61] The analysis, however, may be advanced with somewhat more rigor by examining the data statistically.

First of all, it should be made clear that the time span to be used for this exercise begins in 1801, with the appointment of the great Chief Justice, John Marshall, and ends in June, 1969, with the retirement of Mr. Chief Justice Warren. Thus, the overall period covers approximately 168 years.[62] The initial demarcation has been limited to 1801 for two reasons. First, the presence of Washington during the initial eight years of the Republic's life caused partisan competition for national office to

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*view*, 4 (April, 1951), 446–468; Robert McCloskey, "Economic Due Process and the Supreme Court: An Exhumation and Reburial," *The Supreme Court Review* (1962) pp. 34–62.

[60] See Philip B. Kurland, *Politics, the Constitution, and the Warren Court* (Chicago: University of Chicago Press, 1970); Arthur S. Miller, *The Supreme Court and American Capitalism* (New York: Free Press, 1968); Lowi, *The End of Liberalism.*

[61] While I have examined the relation between judicial decision making and the party systems during maintaining periods of stable partisan competition, Adamany has focused upon the relationship between the Court and the elected branches during realignment phases. Adamany, "Legitimacy, Realigning Elections, and the Supreme Court," pp. 820–843. His historical survey tends to substantiate the thesis advanced here, although the conclusions which he draws from his analysis differ from my own.

[62] To be perfectly precise, the time span from Marshall's appointment on January 31, 1801, to Warren's retirement on June 23, 1969, is 168 years, 4 months, and 23 days.

remain *sub rosa*, and the party system itself had little chance to develop. The two elections of Washington and the election in 1796 of Adams are so exceptional as to defy classification under the S.R.C. scheme.[63] Second, until the appointment of Marshall to the bench, the Supreme Court was hardly a coordinate branch of the government. It handled very few cases and was so lacking in power and prestige that many men of distinction refused to serve on it.[64] With Marshall's appointment and Jefferson's inauguration, however, both the Court and the party system appear to have become well enough established that the electoral classification scheme can be applied to the study of both with some degree of validity. Since both of these events occurred in 1801, that date has been selected as the starting point. Any statistical advantage which may be gained by chopping off the first twelve years of the nation's history is probably lost by carrying the analysis through the 1968 Term of Court. If we are in a realigning phase, then at least some of the seventeen cases decided since 1960 in which the Warren Court declared federal legislation to be unconstitutional should be counted as coming within a transitional period. Because the S.R.C. classification scheme is not predictive, however, these cases have been counted as occurring during a period of stable party competition. If anything, this should skew the data against the hypothesis even more than the elimination of the 12 years from 1789 to 1801 would have biased the analysis in its favor.

According to the way in which I count the cases, there have been ninety-four during this 168-year period which are of interest to us, i.e., cases in which the Court declared federal legislation to be unconstitutional either in whole or in part.[65] Table 2 presents the total incidence of the Supreme Court's exercise of its power of judicial review in relation to the realignment phases identified by Chambers and Burnham utilizing the Survey Research Center's electoral classification scheme. Absolute figures, however, do not tell us very much. Since we are dealing here with a case of concurrent variation in a paired time series, what is required is a measurement of central tendency. During realignment phases or, as they might be called, critical periods, was the tendency of the Court to declare federal legislation unconstitutional significantly greater than during noncritical periods of stable party competition? To answer that question, all that is necessary is to calculate the arithmetic mean for the total years of critical and noncritical periods.[66] The two indexes for all Supreme Court cases declaring acts of Congress unconstitutional are shown in Table 2. As expected, the index for the critical periods is somewhat larger than the index for the noncritical periods, but the difference is not particularly startling. On that basis, one might conclude that during critical periods the Court was slightly more likely to behave in a counter-majoritarian manner—but not very much more likely.[67]

**Table 2. Incidence of all Supreme Court Cases Declaring Congressional Legislation Unconstitutional**

| | Total Years | Number of Cases | Index |
|---|---|---|---|
| Critical periods[a] | 32 | 21 | 0.66[c] |
| Noncritical periods[b] | 136 | 73 | 0.54[d] |
| Coefficient of Counter-Majoritarianism[e] | | | 1.2 |

[a] As identified by Chambers and Burnham. Chambers and Burnham identify four "critical periods" of partisan realignment; while each of these realignment phases is not precisely eight (8) years in length, the author has adopted eight-year periods for purposes of statistical simplicity and comparability. This period, obviously divisible by four, is long enough to allow for broad-scale partisan realignment to manifest itself, yet still short enough to be meaningful within the context of this study.

[b] As identified by Chambers and Burnham.

[c] The index is obtained by calculating the ratio of years of critical periods to the total number of cases within those periods to obtain the central tendency.

[d] The index is obtained by calculating the central tendency of the ratio of years of noncritical periods to the number of cases declaring federal legislation to be unconstitutional within those periods.

[e] The coefficient is obtained by dividing the index for critical periods by the index for noncritical periods, thus arriving at a single number.

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[63] Indeed, Sellers, "The Equilibrium Cycle," classifies the two elections of Washington as "deviating" elections, a classification which poses certain logical difficulties.

[64] See Robert Steamer, *The Supreme Court in Crisis: A History of Conflict* (Boston: University of Massachusetts Press, 1971), pp. 3–23.

[65] Appendices fully elaborating the data base are available from the author upon request.

[66] More sophisticated statistical techniques such as scatter-diagrams could, of course, be employed to make the same point. At this very initial level of analysis, however, I have opted to keep the statistics as simple as possible. Refinements can, of course, always be introduced later.

[67] Candor compels the admission that there is a general lack of dispersion of cases across the four critical periods. As an examination of the appendices demonstrates, the majority of them occurred during the 1928–1936 realignment. While the New Deal's difficulties with the Court were extraordinary, however, one must remember that the Court's opportunities to exercise its power of judicial review were enhanced by a burgeoning population, the vast increase in congressional legislation, and the Court's expanding docket. See Alexander Bickel, *The Caseload of the*

By using *all* cases in which federal legislation has been held unconstitutional in whole or in part, however, we are not really measuring what we set out to measure. For example, in *Myers* v. *U. S.*, decided in 1926, the Court declared unconstitutional an act which had been passed fifty years earlier.[68] In other words, here we have a Court on the eve of the realignment of 1928–36 declaring void a statute passed many years before the realignment of 1888–1896. What can one conclude from this case about the relationship between the *Myers* Court and the law-making majority? Obviously, nothing. Therefore, as did Dahl,[69] let us confine ourselves to cases which held provisions of federal legislation to be unconstitutional within four years of their enactment. We may then assume with a fair degree of confidence that the law-making majority which passed the legislation is not a dead one and that, if the Court acts against that sort of majority it is in fact acting counter to prevailing majority sentiment.

Within the 168 years with which we are concerned, there have been 38 cases in which the Court has declared provisions of federal legislation unconstitutional within four years of enactment.[70] (See Table 3.) Sixteen have occurred during the 32 years of partisan realignment identified by Chambers and Burnham as critical periods. Twenty-two have occurred during the other 136 years. If one now calculates the central tendencies of the Court, one arrives at an index for critical periods of 0.50 and an index for noncritical periods of only 0.16. Which is to say that, as hypothesized, there has been a significantly greater tendency on the part of the Court during realigning periods to utilize its power to declare federal legislation unconstitutional, i.e., to oppose the law-making majority will, than during periods of political stability, when the Court is part of the dominant political majority.

**Table 3. Incidence of Supreme Court Cases Declaring Congressional Legislation Unconstitutional Within Four Years of Enactment**

| | Total Years | Number of Cases | Index |
|---|---|---|---|
| Critical periods | 32 | 16 | 0.50 |
| Noncritical periods | 136 | 22 | 0.16 |
| Coefficient of Counter-Majoritarianism | | | 3.1 |

How much greater? To answer this question we need but to compute a measure of relative variability. The simplest statistical measure of relative variability is, of course, the coefficient of deviation. This coefficient, which for want of a better title, I shall label in this case the Coefficient of Counter-Majoritarianism, is obtained by dividing one index by the other. Calculating the Coefficient of Counter-Majoritarianism for Table 3, we arrive at a figure of 3.1. In other words, statistically-speaking the Court has been more than three times as likely to declare recently enacted federal legislation unconstitutional during the realignment periods identified by Chambers and Burnham than it has been during the vast majority of its history.

Calculation of the Coefficient of Counter-Majoritarianism allows another important comparison. Examining Tables 2 and 3, one finds that using Chambers and Burnham's classifications to plot all of the 38 cases which held federal legislation to be unconstitutional within four years of enactment produces an index for critical periods of 0.50; on the other hand, using Chambers's and Burnham's scheme to plot all of the 94 cases which have ever held federal legislation unconstitutional yields an index for critical periods of 0.66. Does this mean that it is preferable for purposes of the demonstration to use all 94 cases? Obviously, no; for by increasing the number of cases one not only slightly increases the index for the critical periods but also one greatly increases the index for the noncritical periods. Because the statistical bases are different, the two indexes for critical periods simply are not comparable. Coefficients of deviation, however, are, and a comparison of the

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*Supreme Court: And What, If Anything, To Do About It* (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973). If one discounts the number of judicial review cases by the total incidence of opportunities presented for the exercise of the power of judicial review, the New Deal era appears to be less unique, though it remains one of great judicial activity.

[68] 271 U.S. 52 (1926).

[69] Dahl, "Decision-Making," pp. 286–291. I have not, however, adopted Dahl's distinction between cases dealing with major policy issues and those dealing with minor, because I am persuaded by criticisms of his attempts in other areas of analysis to distinguish between important and unimportant policy matters as definitional sleight of hand subject to serious theoretical reservations. See Peter Bachrach and Morton Baratz, "Decisions and Nondecisions: An Analytical Framework," *American Political Science Review*, 57 (September, 1963), 641–652; and "Two Faces of Power," *American Political Science Review*, 56 (December, 1962), 947–952. But see Robert Dahl, *Who Governs?* (New Haven, Conn.: Yale University Press, 1961). For purposes of this study, rejection of such a distinction does sacrifice historical nuance. But, as my conclusions indicate, I do not mean to discourage systemic, historical analysis oft he Court —indeed, quite the opposite—and I hope that scholars who undertake such studies will have better luck than has Dahl in operationalizing definitions of "major" and "minor" policies.

[70] See note 65.

Coefficients of Counter-Majoritarianism in Tables 2 and 3 suggests a significant consideration about Supreme Court behavior. The Coefficient of Counter-Majoritarianism in Table 2 suggests that the Court is slightly more likely to exercise its power of judicial review during realignment phases than during noncritical periods. The Coefficient of Counter-Majoritarianism in Table 3 means that during realignment periods the Court is 3.1 times as likely to declare recently enacted federal legislation unconstitutional than at other times. In relation to each other, however, the two coefficients suggest that during realignment periods the Court is more than two and one-half times as likely to declare recently enacted legislation unconstitutional than other legislation.[71] In other words, it is not merely that during critical periods of partisan realignment the Court is more likely to declare recently enacted federal legislation unconstitutional than at other times, but that during realigning periods this is the very sort of congressional legislation which is most likely to be nullified by the Court!

One might suppose that this finding would justify acceptance of the Dahl-Dooley hypothesis. But a further modification may elaborate the point. Just as the scope of cases which would be accepted as evidence that the Court was out of line with a prevailing majority was narrowed by limiting it to those cases in which legislation not more than four years old was held unconstitutional, the scope of the critical periods may be expanded. If the Court is out of line with the will of the law-making majority during realignment periods, it would be reasonable to expect that, as the realignment takes place and the newly dominant coalition begins to consolidate its position, the Court will gradually come back into line with that majority by virtue of the recruitment process; or, put in statistical terms, the Coefficient of Counter-Majoritarianism will decrease over time.

In order to examine this proposition, it is necessary to introduce the concept of a lag period.[72] We have been examining the behavior of the Court only during realigning periods when the Court, because of the life tenure of the Justices, is out of step with the dominant trends of the political times. During and immediately following these realigning periods, however, appointments are being made to the Court which serve to bring it back into line with the dominant political coalition. If we take as our definition of "lag period" that period of time which is required for a new majority to be appointed to the Court, we have the possibility of calculating two different lag periods. The first possibility is to calculate the lag periods as ending with the fifth or majority appointment to the Court after the *beginning* of the realigning period. Using this possibility, the lag periods end in 1835, 1864, 1896,[73] and 1938.[74] The second possibility is to calculate the lag periods as ending with the fifth or majority appointment to the Court after the *end* of the realignment period. Using this possibility, the lag periods end in 1836, 1864, 1909, and 1940.[75] If the hypothesis is valid, we would expect that the Coefficient of Counter-Majoritarianism would be greatest when one uses as the measure of critical periods only the periods of partisan realignment. The coefficient should be smallest when one uses as the measure of critical periods the expanded eras produced by modifying the periods with the second possible and longest lag period. And it should be somewhere in between these two using the first possible calculation of lag period. In other words, one would suspect that the greater the length of the critical periods, the more the Coefficient of Counter-Majoritarianism would decrease as the Court comes back into line with the dominant law-making majority.

In fact, this is exactly what we do find. Table 4 presents the findings when all cases in which congressional legislation was held unconstitutional within four years of its enactment are plotted against Chambers's and Burnham's critical periods of partisan realignment with the critical

**Table 4. Incidence of Supreme Court Cases Declaring Congressional Legislation Unconstitutional Within Four Years of Enactment (Utilizing Modification No. 1)**

| | Total Years[a] | Number of Cases | Index |
|---|---|---|---|
| Critical periods | 45 | 16 | 0.36 |
| Noncritical periods | 124 | 22 | 0.18 |
| Coefficient of Counter-Majoritarianism | | | 2.0 |

[a] The expanded critical periods are obtained by using the total years of partisan realignment phases (see Table 2) plus the total number of years necessary for a fifth or majority appointment to be made to the Court *after the beginning* of each of the realignment phases.

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[71] 3.1/1.2 = 2.58

[72] For a more extensive discussion of the time lag between the coming of new party eras and the restructuring of Supreme Court policy making, see Dahl, *Pluralist Democracy,* pp. 154–170.

[73] The fifth appointment to the Court made after the beginning of the realignment period, 1888, was made in 1894; thus there is no "lag period." Many students of the Court have observed that in its decision making during this period the Court actually seemed to be anticipating the election returns. This pattern of recruitment suggests why.

[74] See note 65.

[75] See note 65.

**Table 5. Incidence of Supreme Court Cases Declaring Congressional Legislation Unconstitutional Within Four Years of Enactment (Utilizing Modification No. 2)**

| | Total Years[a] | Number of Cases | Index |
|---|---|---|---|
| Critical periods | 61 | 20 | 0.33 |
| Noncritical periods | 107 | 18 | 0.17 |
| Coefficient of Counter-Majoritarianism | | | 1.9 |

[a] The expanded critical periods are obtained by using the total years of partisan realignment phases (see Table 2) plus the total number of years necessary for a fifth or majority appointment to be made to the Court *after the end* of each of the realignment phases.

periods extended by using the first, shortest lag periods. Table 5 illustrates the results produced when the very same cases are plotted against the same classification scheme, with the exception that the critical periods have been extended utilizing the second possible definition of "lag period." In short, during periods of electoral and partisan realignment, the Court, as a result of its life tenure, is most likely to be out of line with the new, dominant law-making majority, which, following Dahl, I have accepted for operational purposes as the index of majority will. As time passes, the new majority is enabled to appoint its own adherents to the Supreme bench, and the Court increasingly returns to harmony with the new law-making majority. Not only is this true logically but also, as the decreasing Coefficients of Counter-Majoritarianism in Tables 3, 4, and 5 illustrate, it is empirically demonstrable. "It is, therefore, accurate to say that the Court will reflect, in an overall, general sense, the values of the dominant political coalition capable of electing the president."[76]

It must be admitted, however, that this study represents but a very tentative first step. Not only does it suggest ways in which concepts and research from other areas of the discipline of political science can be utilized to address one of the classic problems in public law, it also illustrates the methodological and conceptual problems involved in such borrowing. Much more work can and should be done in this area. Different and more sophisticated statistical techniques might be employed to analyze the data presented here; different definitions of realignment periods could be utilized; and Supreme Court behavior in relation to different categories of cases might be correlated with the electoral realignment data.

Adopting different definitions of realignment phases might involve either the refinement of the periods identified here or the use of other scholars' election classifications.[77] Initial tests using some of these different classification schemes confirm that, while the numbers naturally vary, the overall pattern identified here and the conclusions thus suggested are the same. Unlike natural scientists, however, political scientists too seldom build directly on other scholars' work. Therefore, in the interests of comparability and replicability, I have confined this study to the periods already utilized by Chambers and Burnham in their study of party development. For purposes of this analysis, moreover, the critical periods of partisan realignment have been arbitrarily defined as being eight years in length simply for the sake of neatness and order and because eight is readily divisible by four, the Survey Research Center's electoral classification scheme being applicable only to presidential elections. There is, however, nothing sacrosanct about eight-year periods, and the realignment phases might be more rigorously defined. But, again, narrowing the time spans covered or juggling a few years about has not significantly altered the general pattern reported here. Indeed, the use here of some years before the elections which brought new coalitions to power as part of the realignment phases may very probably have artificially deflated the indexes for the critical periods and, thus, the Coefficients of Counter-Majoritarianism in Tables 3, 4, and 5. While it is reasonable to expect tension between a Court of old coalition Justices and a new coalition president and Congress, with the old coalition in power, as in 1928–1932, one would not expect the enactment of legislation which would provoke a Supreme Court declaration of unconstitutionality.

Perhaps future research might attempt to determine the year of ascendancy of the new coalition. One might, for example, define this as possession of the presidency and then use as the critical periods the time between the election of the new coalition president and his appointment of a presumably sympathetic majority of Justices. Using these as realignment years for the Court, one would find results not unlike those reported here. Reckoning ascendancy by control of Congress would not change this much. Such attempts to determine precise years of ascendancy, however, not only must contend with the scholarly contentiousness about which elections actually involved realignments but also will be liable to the charge that they have defined the critical periods so as to inflate the statistical results. Again, this study has used Dahl's cases and Chambers and Burnham's classifications in order to achieve a more integrated analysis. If the results reported here can be achieved by using realignment phases not inherently favorable to the thesis, they may perhaps be considered to be even more persuasive.

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[76] Richard Cortner and Clifford Lytle, eds., *Modern Constitutional Law* (New York: Free Press, 1971), p. 12.

[77] See note 22.

In a more substantive vein, judicial review decisions are only one of several possible indices of the relationship between Supreme Court value structures and philosophies predominant in the larger political processes.[78] Court decisions challenging presidential actions, for example, might be examined within the framework of the S.R.C. model. The late Mr. Justice Robert Jackson observed that

> [The Court] has been in angry collision with most dynamic and popular Presidents in our history. Jefferson retaliated with impeachment; Jackson denied its authority; Lincoln disobeyed a writ of the Chief Justice; Theodore Roosevelt, after his Presidency, proposed recall of judicial decisions; Wilson tried to liberalize its membership; and Franklin D. Roosevelt proposed to "reorganize" it.[79]

But note that at least three of these presidents have been the products of realigning elections.[80] Similarly, despite the operational and conceptual difficulties involved,[81] some scheme might be devised for analyzing the Court's exercise of its power to annul state legislation. In this vein, *Worcester* v. *Georgia*,[82] the only case in which a president apparently ever refused outright to execute a Supreme Court decision and one which occurred immediately following a realignment period, coupled with recent efforts to develop impact typologies of Supreme Court decisions,[83] is highly suggestive of avenues for future research.

Ultimately, the subject calls for an extended, systematic, historical examination of the Court's decision making explicitly based upon the electoral classification scheme devised by the S.R.C. and elaborated by others, a treatment of the Court similar to that which Chambers and Burnham have done for the parties. The politically dominant values of each period must be identified and compared with judicial opinions through in-depth content and dialectical analyses to determine if the statistically apparent "fit" is present in substance.[84] Any study such as this, based upon aggregate data, necessarily sacrifices situational focus for numerological standardization, but Supreme Court decisions are not fungible goods. Indeed, it is striking that some of those decisions which occasioned the loudest protest in their own day and which appear frequently in the casebooks today did not occur during critical periods of partisan realignment.[85] They are included in the casebooks, of course, for reasons other than the public furor which they provoked, and such popular reaction need not invalidate the view that the Court tends to reflect the dominant political values. For an unexpected reversal by a presumed ally would occasion an intense and bitter response, a sort of "stabbed-in-the-back" syndrome. But whether such a speculative interpretation is justified can be known only through careful, historical case analysis focusing upon the political facts of individual cases.

## Conclusion

The question remains, of course, apart from being an interesting statistical exercise, what significance this has for an understanding of the judicial function. First of all, it suggests that Professors Dahl and Charles Black have been correct in emphasizing the Court's function as a legitimating agency.[86] The traditional concept of the Court as the champion of minority rights against majority demands is *largely* incorrect.[87] In order to bring theoretical concepts of the Court's function into line with empirical reality, emphasis should be placed upon the Court's "yea-saying" power rather than upon its "nay-saying" power. Put more cryptically, *McCulloch* v. *Maryland*[88] is the most important case in American constitutional law—not *Marbury* v. *Madison.*[89] Historically, of course, the "nay-saying" power, the power of judicial review, has attracted more scholarly attention because it is so unique. But in both quantitative and qualitative terms the "nay-saying" power is the less important aspect of Supreme Court policy making.

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[78] See, for example, Kurland, *Politics, the Constitution, and the Warren Court*, p. 26. This analysis has, of course, been limited to cases involving the constitutionality of federal statutes for purposes of comparability with Dahl.

[79] Robert Jackson, *The Struggle for Judicial Supremacy* (New York: Knopf, 1941), pp. ix-x.

[80] Indeed, if one uses Sellers's categorization, five of the six Presidents mentioned by Jackson have come to office on the crest of realigning elections. See Sellers, "The Equilibrium Cycle." In this regard, President Nixon's attacks upon the Court's policy making and his attempts to change it while in office are yet other intriguing data possibly indicative of contemporary electoral realignment. See also Duane Lockard, *The Perverted Priorities of American Politics* (New York: Macmillan, 1971), pp. 234–235.

[81] See note 15.

[82] 6 Peters 515 (1832).

[83] See Stephen Wasby, *The Impact of the United States Supreme Court: Some Perspectives* (Homewood, Ill.: Dorsey Press, 1970.)

[84] The forthcoming, multivolume Oliver Wendell Holmes Devise History of the Supreme Court of the United States, under the general editorship of Professor Paul Freund, may greatly advance this task. Given the lawyerly orientation of most of the authors, however, there is little reason for encouragement that what is here advocated will either be attempted or achieved. Indeed, the first two volumes to appear, while comprehensive, have received mixed reactions. See Julius Goebel, Jr., *Antecedents and Beginnings to 1801*, The Oliver Wendell Holmes Devise History of the Supreme Court of the United States, Vol. I (New York: Macmillan, 1971), reviewed by Morton Horowitz, *Harvard Law Review*, 85 (March, 1972), 1076–1082; Charles Fairman, *Reconstruction and Reunion 1864–88: Part One*, The Oliver Wendell Holmes Devise History of the Supreme Court of the United States, Vol. VI (New York: Macmillan, 1971), reviewed by Morton Keller, *Harvard Law Review*, 85 (March, 1972), 1082–1088.

[85] For example, Hepburn v. Griswold, 8 Wallace 603 (1870); Hammer v. Dagenhart, 247 U.S. 251 (1918); Bailey v. Drexel Furniture Co., 259 U.S. 20 (1922). *Hammer* and *Bailey*, however, might be seen as coming within a critical period, depending upon how one views the Wilson era. See Sellers, "The Equilibrium Cycle."

The "yea-saying" power, on the other hand, involves two aspects. The Court can either expand or it can confine or mitigate the operation of statutes. This means that students of judicial policy making should be paying more attention to the Court's statutory, as distinct from its constitutional, interpretation. In the areas of selective service,[90] welfare,[91] and civil rights,[92] to cite but a few recent examples, the Court has been making public policy of tremendous consequence, though without raising any issues of constitutional dimensions. This policy making, however, has tended to escape most political scientists, if not attorneys.[93] Confining the attention to judicial findings of unconstitutionality ignores, as did Dahl, the decisions most pertinent in practice to the questions of whether and how much the Court has succeeded in imposing its own will in the place of Congress's. A systematic study of statutory interpretation might very well yield richer results.

This, in turn, suggests that students of the judicial process should stop equating "activism" with "judicial creativity." Several years ago, Mr. Justice Felix Frankfurter delivered an address in which he claimed that John Marshall had actually been a practitioner of judicial restraint.[94] The reaction of the legal profession was one of general hilarity. Everyone *knew* Marshall had been an activist. But, according to a more complex way of looking at the judicial function, was not Frankfurter correct? After all, in thirty-five years on the bench, Marshall declared only one portion of one fairly limited congressional statute to be unconstitutional. But he was a *very creative* judge.

Finally, however, it must be emphasized that this analysis suggests that the traditional concept of the Court as the protector of minorities is not entirely incorrect. The debate between the judicial activists and the advocates of judicial restraint is particularly relevant during transitional phases of partisan realignment. In fact, by focusing the debate on these periods of political instability, one actually heightens the saliency of that debate, making it more crucial for both the Court and the country. But most of the time that debate will be irrelevant to the actual configurations of political power within the American constitutional system. As Professor Philip B. Kurland has argued,

> There is at least one lesson to be derived from the history of the [Court] . . . and that is that the equation cannot be drawn—as it so frequently is drawn—between "activist" and "liberal" or between "judicial restraint" and "conservative." An "activist" Court is essentially one that is out of step with the legislative or executive branches of the government. It will thus be "liberal" or "conservative" depending upon which role its prime antagonist has adopted. . . .
>
> There is obviously little merit in rehearsing the arguments about whether it was intended to grant the Supreme Court the power of judicial review over national legislation, Even the question of the scope of that authority, which is still much mooted, puts undue emphasis on what has not been a central question.

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[86] Dahl, "Decision-Making"; Charles Black, *The People and the Court: Judicial Review in a Democracy* (New York: Macmillan, 1960). An excellent critique of Black's work is to be found in Kurland, *Politics, the Constitution, and the Warren Court,* pp. 34–39. In spite of his criticisms, however, Professor Kurland also endorses bv implication the legitimation thesis. See Kurland, p. 56. But see Adamany, "Legitimacy, Realigning Elections, and the Supreme Court."

[87] It must be emphasized that many questions in American politics and certainly many which reach the Supreme Court do not resolve themselves into clear-cut issues of a majority versus a minority. Rather, in a system of "minorities rule," many political issues involve conflict between two, competing minorities. In such situations, however the Court strikes the balance, it will by definition advance the rights of a minority. See generally Robert Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956).

[88] 4 Wheaton 316 (1819).

[89] 1 Cranch 137 (1803).

[90] For example, United States v. Seeger, 380 U.S. 1 (1965); Oestereich v. Selective Service, 393 U.S. 233 (1968); Gutknecht v. United States, 396 U.S. 295 (1970); Breen v. Selective Service, 396 U.S. 460 (1970); Welsh v. United States, 398 U.S. 333 (1970).

[91] Townsend v. Swank, 404 U.S. 282 (1972); Jefferson v. Hackney, 406 U.S. 535 (1972); Carleson v. Remillard, 406 U. S. 598 (1972).

[92] For example, Daniel v. Paul, 395 U.S. 298 (1969); Sullivan v. Little Hunting Park, Inc., 396 U.S. 229 (1969); Moose Lodge v. Irvis, 407 U.S. 163 (1972).

[93] But see Martin Shapiro, *Law and Politics in the Supreme Court* (New York: Free Press, 1964), pp. 75–173, 253–327; and *The Supreme Court and Administrative Agencies* (New York: Free Press, 1967); Richard Funston, ed., *Judicial Crises: The Supreme Court in a Changing America* (Cambridge, Mass.: Schenkman, 1974), p. v.

[94] Felix Frankfurter, "John Marshall and the Judicial Function," *Harvard Law Review,* 69 (December, 1955), 217–238.

> For the fact of the matter is—whatever the romance may be—that so far as the relations between Court and Congress are concerned, the invalidation of national legislation has proved historically to be neither so important nor so exacerbating of the differences between the two branches as law professors would make it.[95]

If, then, the Court is normally in line with popular or, at least, law-making majorities, what function could it possibly serve? Traditional theory had a role for the Court to play, the protector of minority rights against the potential tyranny of the majority of the moment. If the Court does not normally play that role, does it serve any function at all which is distinctive to it? Because, if it does not, it is an absurd example of governmental "featherbedding" and should be abolished. In fact, it may be argued that the Court does play a role that differs from the legislative and executive functions and is peculiarly suited to its capabilities. That argument rests on the distinction between expediency and principle and begins with the premise that all governmental actions have two kinds of effects. The first is their immediate, intended effects. For example, faced with a major national economic crisis, we pass legislation which infringes upon agreements entered into between private individuals, i.e., contracts. The second kind of effect of any public program or policy is its bearing upon principles or values which we believe to be fundamental and of general applicability; or, to take our example again, we believe in the right of the majority to determine the course of government and in the right of the nation to protect its existence in times of crisis, either military or economic.[96] We are all able to perceive the relation between our immediate needs or desires and a particular governmental action, and it is these perceptions which we transmit, by virtue of the electoral process, to our representatives. But we are not always able to perceive, because we do not always care about, the relation of a given policy, program, or action to our long-term values.

Assuming that we wish to have principled government, some agency must, in the normal operation of the policy process, be primarily concerned with society's fundamental, underlying values; and practical, if not philosophic, reasons dictate that this agency be one or another of the branches of the federal government. Which one? I submit not only that it should be but also that, in the American constitutional system, it can only be the Supreme Court.[97] The Court, by virtue of its institutional position, is able to deal with matters of principle, whereas Congress and the president, because they are responsible to the electoral whims of the moment, cannot. The Justices "have, or should have, the leisure, the training, and the insulation to follow the ways of the scholar in pursuing the ends of government."[98] This situation and the marvelously functional mystique of the judicial process—those seemingly esoteric institutional customs—allow the Court to appeal to our principled good sense, which may be forgotten in the heat of the moment. It is true, as Dahl points out, that the Court cannot withstand us when we are determined, but through the exercise of its power of judicial review it may entreat us to take a sober second thought about the course which we have set for ourselves.

Perhaps the argument can be cast in comparative, even metaphorical, terms. In communist systems, it is the work of the theoretician to reinterpret Marx so as to bring his philosophy and the immediate acts of the government into accord. With respect to the Constitution, the Supreme Court is the theoretician of American democracy. Or, to cast another metaphor, Louis Heren, formerly Washington correspondent of the *London Times*, has compared the Court's functional relationship to the policy-making agencies of American government with that of the Church to the medieval monarch.[99] It is the Court's function to legitimate, if possible, contemporary public acts by demonstrating their consonance with fundamental constitutional principles. It was Mr. Justice Oliver Wendell Holmes who perhaps best expressed the significance of this function:

> The past gives us our vocabulary and fixes the limits of our imagination; we cannot get away from it. There is, too, a peculiar logical pleasure in making manifest the continuity between what we are doing and what has been done before.[100]

The Court, then, may be conceived of as an educational institution. The Justices of the Supreme Court, as Professor Eugene Rostow has put it, "are inevitably teachers in a vital national seminar."[101] They are in a better position to conduct

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[95] Kurland, *Politics, the Constitution, and the Warren Court*, pp. 17–18, 22. See also Schubert, *Judicial Policy-Making*, pp. 145–157.

[96] See Home Building & Loan Assoc. v. Blaisdell, 290 U.S. 398 (1934).

[97] See also Bickel, *The Least Dangerous Branch*, pp. 23–28; Wechsler, "Neutral Principles."

[98] Bickel, *The Least Dangerous Branch*, pp. 25–26.

[99] Louis Heren, *The New American Commonwealth* (New York: Harper & Row, 1968), p. 18.

[100] Oliver Wendell Holmes, *Collected Legal Papers* (New York: Harcourt, Brace and Howe, 1920), p. 139.

[101] Eugene Rostow, *The Sovereign Prerogative: The Supreme Court and the Quest for Law* (New Haven, Conn.: Yale University Press, 1962), pp. 167–168. Compare Rostow's analogy with Lowi, *The End of Liberalism*, p. 314:

> In the United States the history of political

such a seminar than any other officials in American government.

Finally, in this respect, it may be noted that this concept of the Court as an institution which brings our short-run, expedient means into accord with our long-run ends or principles is also grounds for justifying the Court's power of judicial review. For what would be the good of a declaration of legitimacy from an institution which had no choice but to validate everything that was brought before it? It is only because the Court has the power to annul that its validation is worthwhile.[102] This argument, derived from Professor Black's legitimation thesis, has seldom received better empirical support than in the inordinate haste with which the Voting Right Acts Amendments of 1970, enfranchising eighteen-year-olds, were brought before the Court for constitutional validation.[103]

Holmes's assessment of Marshall, then, may be extended to the Court *qua* institution: Its significance is essentially symbolic.[104] Its most striking power, judicial review, is largely, if not solely, symbolic and is merely the logical corollary of its power to legitimate.[105] As Lord Bryce would have it, the Court derives its authority by being "the living voice of the Constitution, the unfolder of the mind of the people . . . ."[106] This view only serves to re-emphasize the relation between the conditions of freedom and a people's "civic culture." As Mr. Justice Jackson so cogently put the matter,

> It is not idle speculation to inquire which comes first, either in time or importance, an independent and enlightened judiciary or a free and tolerant society. Must we first maintain a system of free political government to assure a free judiciary, or can we rely on an aggressive, activist judiciary to guarantee free government? While each undoubtedly is a support for the other, and the two are frequently found together, it is my belief that the attitude of a society and of its organized political forces, rather than its legal machinery, is the controlling force in the character of free institutions.[107]

Some may discount the importance of this observation. They would do well, however, to recall another of Holmes's dicta:

> At this time we need education in the obvious more than the investigation of the obscure.[108]

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> theory since the founding of the Republic has resided in the Supreme Court. The future of political theory lies there too.

Thus, the legitimation thesis does not necessarily depend, as Professor Adamany claims, upon popular acceptance of the myth of a self-interpreting Constitution. See Adamany, "Legitimacy, Realigning Elections, and the Supreme Court," p. 797. It is for the Court to teach—to convince—that the policies of the dominant coalition are within the broad mainstream of American constitutional values.

[102] See Black, *The People and the Court,* p. 53.

[103] Oregon v. Mitchell, 400 U.S. 112 (1970).

[104] Holmes, *Legal Papers,* p. 270.

[105] See Thurman Arnold, *The Symbols of Government* (New Haven, Conn.: Yale University Press, 1935); Edward S. Corwin, "The Constitution As Instrument And As Symbol," *American Political Science Review,* 30 (December, 1936), 1072–1085; Max Lerner, "Constitution and Court as Symbols," *Yale Law Journal,* 46 (June, 1937), 1291–1319. One of the more interesting aspects of these studies of the symbolic functions of the Court and the Constitution is their dates of publication, no doubt owing largely to the Court's travails with the New Deal. Since the 1930s the study of the symbolic uses of constitutional adjudication appears to have fallen into desuetude. For example, Murray Edelman's admirable analysis of *Politics as Symbolic Action* (Chicago: Markham, 1971) contains not even a passing reference to the judicial process.

[106] James Bryce, *The American Commonwealth,* 2nd ed., rev. (New York: Macmillan, 1891), I, 348.

[107] Robert Jackson, *The Supreme Court in the American System of Government* (New York: Harper & Row, 1955), p. 81.

[108] Holmes, *Legal Papers,* pp. 292–293.

# Determinants of the Outcomes of Midterm Congressional Elections*

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The outcomes of midterm congressional elections appear as a mixture of the routine and the inexplicable. In every off-year congressional election but one since the Civil War, the political party of the incumbent President has lost seats in the House of Representatives. Yet the factors explaining the variation around the usual aggregate outcome of midterms are not well understood; indeed, in *Politics, Parties and Pressure Groups*, V. O. Key suggested that the nature of the midterm verdict lacked explanation in any theory of a rational electorate:

> Since the electorate cannot change administrations at midterm elections, it can only express its approval or disapproval by returning or withdrawing legislative majorities. At least such would be the rational hypothesis about what the electorate might do. In fact, no such logical explanation can completely describe what it does at midterm elections. The Founding Fathers, by the provision for midterm elections, built into the constitutional system a procedure whose strange consequences lack explanation in any theory that personifies the electorate as a rational god of vengeance and of reward.[1]

Furthermore, the central facts of midterm elections—the almost invariable loss by the President's party combined with the great stability in partisan swings compared to on-year elections[2]—both suggest an electorate returning to their normal partisan alignment after the more hectic presidential contest two years earlier, rather than an electorate responding to short-term national forces and acting as a "rational god of vengeance and of reward." In seeking to explain the sources of midterm loss, both Campbell and Key emphasized the differences in turnout in off-year compared to on-year elections—rather than short-run factors such as the electorate's evaluation of the performance of the President and his party.[3] Following up this approach, Hinckley assessed the administration's midterm loss with reference to the prior presidential election and concluded: ". . . the midterm 'referendum' appears quite derivative. It is, in part, a continuation of the verdict expressed in the preceding presidential elections and, in part, an adjustment of that verdict, an adjustment built into the midterm by the preceding presidential election."[4] Another recent analysis of midterms from 1954 to 1970 concludes that they are "non-events" and "non-elections," predictable solely from the preceding presidential election.[5] Finally, the Stokes-Miller study of the

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* I wish to thank Marge Cruise, Jan Juran, Alice Anne Navin, Susan Spock, Michael Stoto, and Richard Sun for their help in the collection and analysis of the data. John L. McCarthy, Richard A. Brody, Gerald H. Kramer, Duane Lockard, David Seidman, and Jack Walker provided advice and encouragement. Financial support came from the Woodrow Wilson School of Public and International Affairs at Princeton University and from a fellowship at the Center for Advanced Study in the Behavioral Sciences. Early drafts of the paper were presented in seminars at the Center (October, 1973); the Bay Area Political Behavior Seminar (January, 1974); and Princeton University (October, 1974). A partial, preliminary version of the model is reported in Edward R. Tufte, *Data Analysis for Politics and Policy* (Englewood Cliffs, New Jersey: Prentice-Hall, 1974), pp. 140–145. I wish also to thank several anonymous reviewers and Dr. Ellen Y. Siegelman of the *Review* for their helpful comments. These individuals and institutions do not, of course, bear responsibility for the faults of the study.

[1] V. O. Key, Jr., *Politics, Parties, and Pressure Groups*, 5th ed. (New York: Thomas Y. Crowell, 1964), pp. 567-568.

[2] Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly*, 26 (Winter, 1962), 531–546.

[3] Angus Campbell, "Voters and Elections: Past and Present," *Journal of Politics*, 26 (November, 1964), 745–757; Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (Fall, 1960), 397–418; and Key, pp. 568–569.

[4] Barbara Hinckley, "Interpreting House Midterm Elections: Toward a Measurement of the In-Party's 'Expected' Loss of Seats," *American Political Science Review*, 61 (September, 1967), 700.

[5] Mark N. Franklin, "A 'Non-election' in America? Predicting the Results of the 1970 Mid-term Election for the U.S. House of Representatives," *British Journal of Political Science*, 1 (October, 1971), 508–513. See also Anthony King, "Why All Governments Lose By-Elections," *New Society*, March 21, 1968, pp. 413–415; Nigel Lawson, "A New Theory of By-Elections," *Spectator*, November, 1968, pp. 651–652; and John D. Lees, "Campaigns and Parties—The 1970 American Mid-Term Elections and Beyond," *Parliamentary Affairs*, 24 (Autumn, 1971), 312–320. The view that midterms represent, in large measure, the electoral swing of the pendulum (or electoral surge and decline) does not seem to be held by politicians. Sam Kernell has compiled convincing evidence that a central premise among American politicians is that "the president's popularity directly affects his congressional party candidates' chances for election" in midterms: Sam Kernell, "Presidential Popularity and Negative Voting: An Alternative Explanation of the Mid-Term Electoral Decline of the President's Party," paper delivered at the 1974 annual meeting of the American Political Science Association. See also the variety of interpretations of the outcome

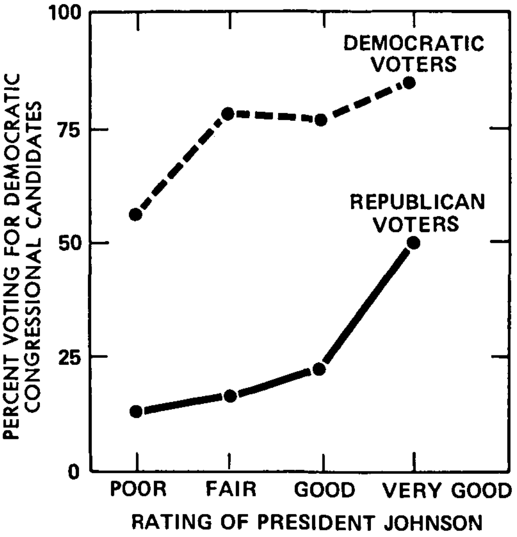
1958 midterm found that voters were simply not responding to the parties' legislative records in casting midterm ballots.

Nevertheless the prevailing view of midterm outcomes as an adjustment restoring the normal partisan equilibrium unrelated to objective events in the two years prior to the midterm is incomplete, for while it explains why the President's party should almost always be operating in the loss column, it does not account for the *number* of votes and seats lost by the President's party. In statistical parlance, the adjustment model of midterm congressional elections explains the location of the mean rather than variability about the mean. But, as Key indicated, "The significance of a specific midterm result comes not from the simple fact of losses by the President's party. Some loss is to be expected. It is the magnitude of the loss that is important."[6]

In this study, we seek to explain the *magnitude of the national midterm loss* by the President's party: why do some presidents lose fewer congressional seats at midterm than other presidents? Do the outcomes of midterm congressional elections represent the electorate's evaluation of the President's performance? Do such outcomes reflect the electorate's evaluation of the administration's management of the economy? If a relatively large proportion of the electorate approves the President's handling of his job or his management of the economy, then does his party lose less in the midterm congressional elections? Or, on the other hand, is the midterm "referendum" only "derivative" and the outcomes lacking in rational explanation? Since those citizens showing up at the polls in the midterm are probably somewhat more politically sophisticated and interested than those voting in on-year elections,[7] the assertion that midterm outcomes are "irrational" provides a substantial challenge to the view that the electorate behaves in rational ways, or at least in ways somewhat responsive to the political environment.

Because there are no other targets available at the midterm, it is not unreasonable to expect that some voters opposed to the President might take out their dissatisfaction with the incumbent administration on the congressional candidates of the President's party. Arseneau and Wolfinger, using survey data, provide some evidence that " . . . the public image of Congress is rather undifferentiated and, moreover, assessments of the two parties' performance are likely to be determined predominantly by evaluations of the president rather than Congress. . . . congressional candidates are likely to suffer or benefit from voters' estimates of how well the president has been doing his job."[8] Figure 1 shows their analysis of the Survey Research Center data for the 1968 election; for voters of both parties, support for Democratic congressional candidates increases by about thirty percentage points as the evaluations of President Johnson go from "poor" to "very good."

**Figure 1. Relationship Between Evaluations of the President and Vote For Congressional Candidate, 1968.**



Source: Arseneau and Wolfinger, "Voting Behavior in Congressional Elections," p. 16.

Thus our first link in the model explaining midterm outcomes is the relationship between the aggregate outcome and the electorate's evaluation of the President at the time of the election: at

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of British by-elections in Chris Cook and John Ramsden, eds., *By-Elections in British Politics* (New York: St. Martin's Press, 1973). Alternative midterm models are discussed in Richard W. Boyd and James T. Murphy, "How Many Seats Will the Republicans Lose? Changes in the House: A Prediction," *New Republic,* October 24, 1970, pp. 12–14.

[6] Key, p. 569.

[7] This conventional description of the midterm electorate has been challenged by the evidence of Robert B. Arseneau and Raymond E. Wolfinger, "Voting Behavior in Congressional Elections," paper delivered at the 1973 annual meeting of the American Political Science Association.

[8] Arseneau and Wolfinger, p. 3. Several other recent studies have greatly reinforced the evidence linking evaluations of the incumbent president to electoral choices in congressional races. See Kernell, "Presidential Popularity and Negative Voting," for an extensive analysis of Gallup poll data in six midterm elections; and James E. Piereson, "Presidential Popularity and Midterm Voting at Different Electoral Levels," *American Journal of Political Science,* forthcoming, November, 1975, which uses data from the 1970 election study conducted by the Center for Political Studies at the University of Michigan.

what rate fluctuations in presidential popularity are translated into fluctuations in votes and congressional seats in off-year elections? Two kinds of evidence help assess the relationship: aggregate data for the whole nation and individual interviews in surveys of the electorate. Although this study is largely based on aggregate data, fortunately some new detailed material at the individual level also bears on the issue.

The second explanatory variable in our analysis of midterm outcomes is the performance of the economy in the year prior to the midterm. There is already available a careful study linking prevailing economic conditions to aggregate electoral outcomes, including midterms. Kramer's model explains 56 per cent of the variation in the national partisan division of vote in the midterms from 1898 to 1962.[9] Although these results have been subjected to vigorous, but not convincing, critiques by Stigler and others, it seems clear from Kramer's analysis that midterm outcomes are responsive to changes in objective economic conditions taking place between the presidential election and the midterm itself.[10]

In summary, our data analysis here will estimate the impact on midterm congressional elections of the electorate's evaluations of presidential performance and of prevailing economic conditions prior to the election. Such estimates lead to predictions of the national partisan division of the congressional vote. That vote, of course, is not the ultimate measure of the midterm outcome —it is the resulting partisan distribution of *seats* in the House of Representatives that matters politically. As we will see, the translation of votes into seats has changed considerably over the period covered in our study: comparable shifts in the midterm partisan division of the vote are now worth less than half as much in terms of congressional seats compared to 35 years ago[11]—thus significantly muting the impact of midterm elections on party alignment in the House. It is therefore not enough to explain and predict the partisan division of the vote; it is necessary also to take into account the changing political consequences of that vote resulting from the changing character of the translation of votes into congressional seats. Thus the model is:

Public approval of President at time of midterm election ↘
Pre-election shifts in economic conditions ↙
Magnitude of national vote loss by President's party
↓
Magnitude of congressional seat loss by President's party

### Measuring the Variables in the Model

A substantial amount of recent research has contemplated the three variables in the first stage of the model: the measurement of economic performance in relation to electoral outcomes, the meaning of the long-run Gallup poll question on approval of the President, and the proper way to interpret midterm congressional outcomes with respect to the vote and the seat loss by the President's party. It is clear from Stigler's study that the results in the aggregate analysis of congressional elections are sensitive to the particular specification of variables in the model. The problem is further complicated by the difficulty of choosing among alternative specifications, given the relatively small number of data points, the high intercorrelation between alternative measures of the same general concept, and difficulty in handling idiosyncratic problems such as third party candidacies, elections that involve unusual factors, and the like. In the face of these problems, some special attention to the particular operationalization of each variable is necessary, even though each seemingly has rather obvious empirical referents.

The most important variable to measure well is the dependent or response variable, the magnitude of the midterm loss by the President's party. As most discussions evaluating midterm losses point out, the idea of "loss" implies the question "Relative to what?"[12] The relevant comparison, it seems, is between the normal, long-run congressional vote for the political party of the current President and the outcome of the midterm election at hand—that is, a standardized vote loss that takes the long-run partisan trend into account:

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[9] Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review,* 65 (March, 1971), 131–143. A data error in this paper is corrected in its Bobbs-Merrill reprint (PS-498); see also Saul Goodman and Gerald H. Kramer, "Commentary on Arcelus and Meltzer, 'The Effect of Aggregate Economic Conditions on Congressional Elections'," *American Political Science Review,* forthcoming; Gerald H. Kramer and Susan J. Lepper, "Congressional Elections," in *The Dimensions of Quantitative Research in History,* ed. William O. Aydelotte, Allan G. Bogue, and Robert William Fogel (Princeton: Princeton University Press, 1972), 256–284; and Susan J. Lepper, "Voting Behavior and Aggregate Policy Targets," *Public Choice,* 18 (Summer, 1974), 67–81.

[10] George J. Stigler, "General Economic Conditions and National Elections," *American Economic Review,* 63 (May, 1973), 160–167; and further discussion by Paul W. McCracken, Arthur M. Okun, and others, pp. 169–180. Stigler's method might best be described as a "most squares" technique: find the specification that maximizes the error variance. But the outstanding work in discovering the pessimum most squares model is Francisco Arcelus and Allen H. Meltzer, "The Effect of Aggregate Economic Conditions on Congressional Elections," *American Political Science Review,* forthcoming; see the reply of Goodman and Kramer.

[11] On the translation of votes into seats, see Edward R. Tufte, "The Relationship Between Seats and Votes in Two-Party Systems," *American Political Science Review,* 67 (June, 1973), 540–554.

[12] Hinckley, "Interpreting House Midterm Elections;" Harvey Zeidenstein, "Measuring Congressional Seat Losses in Mid-Term Elections," *Journal of Poli-*

$$\begin{array}{l}\text{standardized vote loss by}\\ \text{President's party in the } i^{\text{th}}\\ \text{midterm election}\end{array} = \begin{array}{l}\text{national congressional vote}\\ \text{for President's party in the}\\ i^{\text{th}} \text{ election}\end{array} - \begin{array}{l}\text{average national congressional}\\ \text{vote for party of current Presi-}\\ \text{dent in previous elections}\end{array}$$

Thus the loss is measured with respect to how well the party of the current President has normally done, where the normal vote is computed by averaging that party's national vote over the eight preceding both on-year and off-year congressional elections.[13] This standardization is necessary because the Democrats have dominated postwar congressional elections; if the unstandardized vote won by the President's party is used as the response (dependent) variable, the Republican presidents would appear to do poorly. For example, when the Republicans win 48 per cent of the national congressional vote, it is, relatively, a substantial victory for that party and should be counted as such. The eight-election standardization takes this effect into account as well as yielding a model with a bit of dynamics to it.[14] It is straightforward, furthermore, to reconstruct the actual or predicted outcome from the standardized vote, thereby permitting pre-election predictions of the partisan division of the vote. There are many alternative ways of quantitatively assessing the midterm loss.[15] The elections used in the standardization could be weighted, with the heaviest weight given to the most recent elections. On-year congressional elections might be discarded altogether. A larger or smaller number of elections might be used. In general, most of the obvious alternatives are highly correlated; in addition, experiments with a variety of methods for computing the normal vote revealed that the model performed well under most reasonable alternatives. Table 1 shows the computations for the midterm elections from 1938 to 1970.

Let us now consider the explanatory variables, the public's approval of the President and the economic conditions prevailing at the time of midterm election.

The only long-run consistent measure of the public's evaluation of the President's general performance is the standard Gallup poll question asked in their monthly surveys: "Do you approve or disapprove of the way [the incumbent] is handling his job as President?"[16] While the Gallup

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*tics,* 34 (February, 1972), 272–276; and A. H. Taylor, "The Proportional Decline Hypothesis in English Elections," *Journal of the Royal Statistical Society,* Series A, 135 (1972), 365–369.

[13] See the normalizations in William H. Flanigan and Nancy H. Zingale, "The Measurement of Electoral Change," *Political Methodology,* 1 (Summer, 1974), 49–82; also William H. Flanigan and Nancy H. Zingale, "Electoral Competition and Partisan Realignment," paper delivered at the 1973 annual meeting of the American Political Science Association. The standard discussion is, of course, Philip E. Converse, "The Concept of a Normal Vote," in Angus Campbell et al., *Elections and the Political Order* (New York: Wiley, 1966), pp. 9–39.

[14] The Democratic loss in the 1938 midterm election is measured relative to the share of votes received by Democratic congressional candidates in the previous *three* elections (1932, 1934, and 1936), rather than the eight used for estimating the normal vote in the other midterms. The eight election normalization fails for 1938 because of the rapid and extensive realignment from 1930 to 1932; thus the averaged results of congressional elections from 1932 on gives a more reasonable estimate of the 1938 normal vote than the inclusion of several Republican dominated years prior to the realignment. The percentage share of the congressional vote received by the Democrats during that period shows the problem:

| | |
|---|---|
| 1938 | 50.8% |
| 1936 | 56.2% |
| 1934 | 56.2% |
| 1932 | 56.9% |
| 1930 | 45.9% |
| 1928 | 42.8% |
| 1926 | 41.6% |
| 1924 | 42.1% |
| 1922 | 46.4% |

From this series, its seems clear that a reasonable estimate of the normal Democratic vote for the 1938 election should be based on the elections of 1932, 1934, and 1936, rather than earlier years.

[15] See Harvey M. Kabaker, "Estimating the Normal Vote in Congressional Elections," *Midwest Journal of Political Science,* 13 (February, 1969), 58–83.

[16] The presidential approval ratings from 1946 to 1970 are from *The Gallup Opinion Index,* 64 (October, 1970), 16; the 1938 approval rate, 57 per cent, was averaged (because of inconsistencies in question wording and survey dates) from two surveys: September, 1938—"Are you for or against Roosevelt today?" 55.2 per cent; and October, 1938—"In general do you approve or disapprove of Roosevelt as President?" 59.6 percent. The source for the 1938 data is George Gallup, *The Gallup Poll* (New York: Random House, 1972), pp. 118, 122. Similar, but not identical figures are reported in a fine study by Wesley C. Clark, "Economic Aspects of a President's Popularity" (Ph.D. dissertation, University of Pennsylvania, 1943), p. 47, which also contains an extensive discussion of the early years of the series. A flawed analysis of the factors affecting the ratings is given in John E. Mueller, "Presidential Popularity from Truman to Johnson," *American Political Science Review,* 64 (March, 1970), 18–34; and in John E. Mueller, *War, Presidents and Public Opinion* (New York: Wiley, 1973). The substantive and statistical difficulties in Mueller's analysis are discussed in Richard A. Brody and Benjamin I. Page, "The Impact of Events on Presidential Popularity: The Johnson and Nixon Administrations," paper delivered at the 1972 annual

**Table 1. Data for Midterm Elections**

| Year | $V_i$ Nationwide Midterm Congressional Vote for Party of Incumbent President | $N_i^8$ Mean Congressional Vote for Party of Incumbent President in 8 Prior Elections | $Y_i = V_i - N_i^8$ Standardized Vote Loss (−) or Gain (+) by President's Party in Midterm Election | $P_i$ Gallup Poll Rating of President at Time of Election | $\Delta E_i$ Yearly Change in Real Disposable Income Per Capita |
|---|---|---|---|---|---|
| 1938 | 50.82% | Democratic 57.18%* | −6.36% | 57% | −$82 |
| 1946 | 45.27% | Democratic 52.57% | −7.30% | 32% | −$36 |
| 1950 | 50.04% | Democratic 52.04% | −2.00% | 43% | $99 |
| 1954 | 47.46% | Republican 49.79% | −2.33% | 65% | −$12 |
| 1958 | 43.90% | Republican 49.83% | −5.93% | 56% | −$13 |
| 1962 | 52.42% | Democratic 51.63% | +0.79% | 67% | $60 |
| 1966 | 51.33% | Democratic 53.06% | −1.73% | 48% | $96 |
| 1970 | 45.68% | Republican 46.66% | −0.98% | 56% | $69 |

* For 1938, mean is based on last three elections only. See note 14.

poll has asked for evaluations of the President since 1935 (thereby limiting our study to the midterms since 1938), the wording of the question has shifted and it was only in 1945 that the standard wording was adopted. The wording used immediately prior to the 1938 midterm, however, differs only slightly from the postwar surveys and consequently our analysis includes the 1938 survey results.[17]

Table 1 shows the approval ratings from the surveys taken prior to each midterm election. The simple correlation between the normalized midterm loss by the party of the President and the pre-election approval rating is .50, indicating that larger losses are associated with lower popularity.

Although we have only a single relatively consistent indicator over the years of the public's evaluation of the President, there are available, on the other hand, many different possible measures for the other dependent variable, the performance of the economy. Neither theory nor data strongly suggest a good choice. The discussions of the studies of Kramer and Stigler by McCracken, Okun, Riker and Ireland reveal many speculations and little theory—beyond the observation, agreed upon by all, that general economic conditions might somehow have an effect on some elections—that suggest specific hypotheses about which economic variables should be important or what kinds of time perspectives voters might use in evaluating the pre-election performance of the economy.[18] Kramer's empirical work has shown the political importance of inter-election shifts in real income and of inflation; both seem to have more impact on congressional elections than do shifts of ordinary magnitude in unemployment.[19] The best measure of economic conditions for our model therefore appears to be pre-election changes in real disposable income per capita.[20] This measure may reflect the economic

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meeting of the American Political Science Association; and in Douglas A. Hibbs, Jr., "Problems of Statistical Estimation and Causal Inference in Time-Series Regression Models," in *Sociological Methodology, 1973–1974,* ed. Herbert Costner (San Francisco: Jossey-Bass, 1974), pp. 252–308.

[17] The details of the shifting wording are in Clark, *Economic Aspects of a President's Popularity*, pp. 47, 55–60.

[18] Stigler's article itself, as well as the commentary, makes clear the lack of theoretical specificity found in current models correlating economic time series to electoral outcomes; see Stigler, "General Economic Conditions and National Elections," pp. 160–167; and further comments, 168–180. Stigler uses a two-year difference to compute the change in economic conditions for his model, on the view that voters compare the change in the economy in the current election year with that at the time of the previous election two years before. That model seems doubtful, attributing an excessively long time perspective to voters, especially for moderate economic changes. Another way to look at the matter is to consider the voter's problem as the generation of an estimate of the rate of real economic change immediately prior to the election—in order to estimate what can be expected with respect to the performance of the economy under the incumbent administration. If those expectations are good, the party of the incumbent President receives a vote; otherwise the out-party gets the vote. If this is a reasonable description of the problem facing the voter, then the voter would probably not use Stigler's method—a two-year difference in economic changes—to estimate expected or immediately past economic performance. A one-year difference is at least slightly more realistic. Additional progress on this question can probably be made by examining survey evidence at the individual level rather than by still more specifications of aggregate models.

[19] Kramer, p. 139; see also Goodman and Kramer.

[20] The yearly change in real disposable personal income per capita (in 1958 dollars) was computed from data in *The Annual Report of the Council of Economic Advisers, 1973* (Washington, D.C.: U.S. Government Printing Office, 1973), p. 213; and *The Annual Report of the Council of Economic Advisers,* 1971, p. 215.

concerns of many voters, for it assesses the short-run shift in average economic conditions, measured in terms of real purchasing power, prevailing at the individual level—a shift in conditions for which some voters might hold the incumbent administration and the political party of the incumbent administration responsible.

In summary, our model explaining the midterm vote received by the political party of the incumbent President is:

$$Y_i = \beta_0 + \beta_1 P_i + \beta_2(\Delta E_i) + u_i \qquad (1)$$

where

$Y_i$ = standardized midterm vote for the political party of the incumbent President in the $i^{\text{th}}$ midterm congressional election, $Y_i = V_i - N_i^8$;

$V_i$ = nationwide share of the two-party congressional vote received by political party of the incumbent President in the $i^{\text{th}}$ midterm;

$N_i^8$ = normal congressional vote for the political party of the incumbent President at the time of the $i^{\text{th}}$ midterm; computed as the average, over the preceding eight congressional elections (both on- and off-years) prior to the $i^{\text{th}}$ midterm, of the nationwide share of the two-party congressional vote received by the political party of the President in office at the time of the $i^{\text{th}}$ midterm;

$P_i$ = percentage of sample in Gallup poll in September prior to the $i^{\text{th}}$ midterm who approve of the job the incumbent President is doing;

$\Delta E_i$ = yearly change in real disposable personal income per capita between the year of the midterm and the previous year; and

$u_i$ = residual or error term.

Note that the model can be rewritten to estimate the nationwide congressional vote for the President's party:

$$V_i = \beta_0 + \beta_1 P_i + \beta_2(\Delta E_i) + \beta_3 N_i^8 + u_i. \qquad (2)$$

Thus, while equation (1) leads to estimates of $Y_i$, the standardized vote loss by the President's party, equation (2) estimates $V_i$, the nationwide proportion of the congressional vote for the party of the President. The two models are identical, however, if $\beta_3$ is unity—for then $N_i^8$ can be moved, in equation (2), from the right-hand to the left-hand side of the equation. The data were used to fit equation (2) and $\beta_3$ was, in fact, very close to unity; and therefore equation (1) will be the model developed in the remainder of the analysis. Equation (1) estimates one less parameter than equation (2), an advantage since the model must be estimated on the basis of a very small number of cases. The small $N$ and the potential fragility of the model also suggest that extraordinary tests of the model's explanatory and predictive capacity are necessary. Many such tests will be conducted, including an assessment of the model in predicting the outcome of the 1974 midterm congressional elections.

We now use the data of Table 1 to estimate the $\beta$'s, the regression coefficients, in the model.[21] The estimate, $\hat{\beta}_1$, assesses the impact of presidential approval rating on the midterm vote; and $\hat{\beta}_2$, the impact of the pre-election change in real disposable personal income per capita on the midterm vote.

### Fitting the Model and Confirming the Results

In order to explain the magnitude of the aggregate loss of votes by the President's party in midterm congressional elections, we estimate our multiple regression model described above (written here in more informal notation):

$$\text{Standardized vote loss by President's party in the midterm} = \beta_0 + \beta_1 \begin{bmatrix}\text{Presidential}\\ \text{popularity}\end{bmatrix} + \beta_2 \begin{bmatrix}\text{Yearly change}\\ \text{in economic}\\ \text{conditions}\end{bmatrix}$$

The idea is, of course, that the lower the approval rating of the incumbent President and the less prosperous the economy, the greater the loss of support for the President's party in the midterm congressional elections.

Table 2 shows the estimates of the model's coefficients. The results are statistically secure since the coefficients are at least four times their standard errors. The fitted equation indicates:

—A change in presidential popularity of 10 percentage points in the Gallup poll is associated with a national change of 1.3 percentage points in national midterm vote for congressional candidates of the President's party.

—A change of $100 in real disposable personal income per capita in the year prior to the midterm election is associated with a national change of 3.5 percentage points in the midterm vote for congressional candidates of the President's party.

The fitted equation explains statistically 91.2 per cent of the variance in national midterm out-

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[21] The midterm of 1942 is omitted from the analysis because of the special effect of wartime controls on the economy and of wartime conditions on evaluations of the incumbent President. Kramer also dropped wartime years; see Kramer, p. 137.

**Table 2. Multiple Regression Fitting Standardized Vote Loss by President's Party in Midterm Elections**

$$Y=\beta_0+\beta_1(P)+\beta_2(\Delta E)$$

| | Regression Coefficient and (Standard Error) | Simple Correlation with Midterm Loss |
|---|---|---|
| Presidential approval rating ($P$) | $\hat{\beta}_1=.133^*$<br>(.033) | .503 |
| Yearly change in real disposable personal income per capita ($\Delta E$) | $\hat{\beta}_2=.035^*$<br>(.006) | .795 |

Constant ($\hat{\beta}_0$) = −11.083.
$R^2=0.912$.
* Statistically significant at the .01 level.

comes from 1938 to 1970;[22] or, to put it another way, the correlation between the actual election results and those predicted by the model is .955, as shown in Figure 2. Since the fitted equation uses two meaningful explanatory variables, it seems reasonable to believe that in this case a successful statistical explanation is also a successful substantive explanation.

Before turning to the substantive consequences of the fitted equation, it is necessary to test the soundness of the model. Such tests are important because the model is based on a relatively short series of elections (although more than many studies of midterms)—and also because the model is apparently so successful in terms of the variance explained. Are the findings the result of some artifact?

The overall equation and the estimates of the individual regression coefficients are statistically significant at the .01 level. Let us consider four additional tests of the model: the independent replication of the estimated regression coefficients, and tests assessing the stability, postdictive quality, and predictive quality of the regression equation.

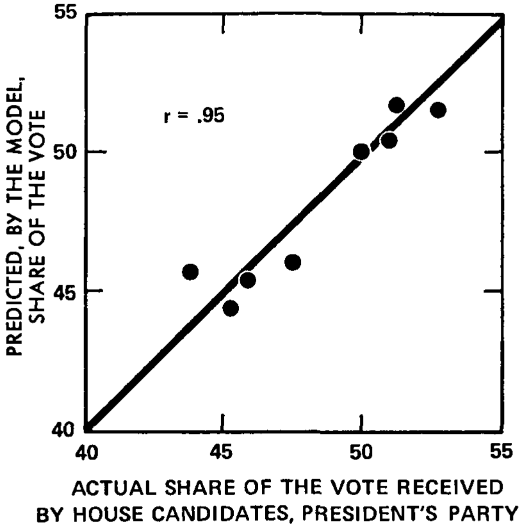

**Figure 2. Actual and Predicted Share of the Two-Party Vote Received by Congressional Candidates of President's Party, Midterm Elections, 1938–1970**

Some independent studies are consistent with estimates of the regression coefficients in this model. Kramer finds that, in congressional elections from 1896 to 1964 (including both on- and off-year congressional elections), "a 10% decrease in per capita real personal income would cost the incumbent administration 4 to 5 per cent of the congressional vote, other things being equal."[23] Since the average real disposable personal income per capita in the period under study here is around $1800, Kramer's model estimates that a shift of approximately $180 in real disposable income would produce a shift of 4 to 5 per cent in the congressional vote. Our regression indicates that a shift of $180 in income would produce a shift of about 6 per cent in the congressional vote. Given the differences between the studies with respect to the period and types of elections covered, the results seem quite comparable. Our short-term (1938–1970) estimate approximately matches Kramer's long-term (1896–1964) estimate of the impact of economic conditions on congressional elections.

An independent confirmation of the estimated effect of the presidential approval level on the midterm outcome comes from a study based on survey interviews from national samples of individual voters. Kernell computed voter defection rates by analyzing responses to the interviews in the Gallup poll's samples prior to the midterms of 1946, 1950, 1954, 1958, and 1962. He finds that "for every nine point change in the percentage approving the president, his party's congressional vote will change 1.4 percentage points."[24] Our estimate, using aggregate data, is virtually identical. The two estimates—one based on individual interviews recording respondent's claimed vote choice in the midterm and the evaluation of the incumbent president, the other based on the aggregate approval rating and the actual vote result—were arrived at completely independently.

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[22] In regressions of this sort, involving such a small number of degrees of freedom, some prefer to use a corrected $R^2$ that takes into account the loss in degrees of freedom as the coefficients are estimated. In our case, the corrected $R^2$ is 0.88. See Carl F. Christ, *Econometric Models and Methods* (New York: Wiley, 1966), pp. 509–510.

[23] Kramer, p. 141.

[24] Kernell, p. 32.

Table 3. After-the-Fact Predictive Error of the Model

| Year | Actual Vote for House Candidates, President's Party | Gallup Poll Prediction* | Model Prediction | Gallup Absolute Error | Model Absolute Error |
|---|---|---|---|---|---|
| 1938 | 50.8 | 54 | 50.8 | 3.2 | 0.0 |
| 1946 | 45.3 | 42 | 44.5 | 3.3 | 0.8 |
| 1950 | 50.0 | 51 | 50.1 | 1.0 | 0.1 |
| 1954 | 47.5 | 48.5 | 46.9 | 1.0 | 0.6 |
| 1958 | 43.9 | 43 | 45.7 | 0.9 | 1.8 |
| 1962 | 52.4 | 55.5 | 51.6 | 3.1 | 0.8 |
| 1966 | 51.3 | 52.5 | 51.7 | 1.2 | 0.4 |
| 1970 | 45.7 | 47 | 45.4 | 1.3 | 0.3 |

Average absolute error, Gallup = 1.9 percentage points.
Average absolute error, Model = 0.6 percentage points.

* National survey taken 7 to 10 days prior to the election. The question asked is "If the elections for Congress were being held today, which party would you like to see win in this congressional district, the Democratic or the Republican party?"

In our small data set, based on single readings taken once every four years immediately prior to each midterm, there is no relationship between the presidential approval rating and the pre-election shift in real disposable income. (Note that the approval rating is measured in absolute terms, rather than as a pre-election shift.) For example, the Eisenhower midterms of 1954 and 1958 reflect a popular president and a mediocre short-run economic performance. More generally, it appears that presidential approval ratings are a function of many different factors, including performance in foreign affairs, the President's personality, scandals, and large downward shifts in economic conditions.

To check the stability of the fitted equation, the model was re-estimated after excluding one election at a time from the computations.[25] The regression coefficients remained very stable and statistically significant, and the $R^2$ did not go below .89 in the re-estimates. It is clear that the estimates for the overall model are not dominated by a single set of outlying values for one election.

As another check of the adequacy of model, its after-the-fact predictions of midterm outcomes were compared with the pre-election predictions made by the Gallup poll in the national survey conducted a week to ten days before each election.[26] As Table 3 shows, the model outperforms the pre-election predictions based on surveys directly asking voters how they intend to vote. Now all this is, of course, after the fact and it would be more useful to have a genuine prediction in hand prior to the election to test the model. This leads to our strongest test of the model—for we can examine its *predictive* powers in a series of historical experiments.

Suppose the model had been estimated prior to the 1970 election, using the data from the midterm elections from 1938 to 1966. The fitted equation prior to the 1970 election was:

$$\text{Standardized midterm loss} = -11.06 + .133(P) + .035\,(\Delta E).$$

Now let us use this model, generated from the experience from 1938 to 1966, to predict the outcome of the 1970 election. The September, 1970, level of presidential approval was 56 per cent; the 1969–1970 shift in real disposable personal income per capita was \$69. Plugging those values into the pre-1970 equation leads to a pre-election forecast of the 1970 outcome:

$$\begin{aligned}\text{Predicted 1970 normalized midterm loss} &= -11.06 + .133(56) + .035(69)\\ &= -1.20 \text{ per cent.}\end{aligned}$$

Since the actual normalized loss was −0.98 per cent, the model performed well in this predictive trial. To translate these results to the actual partisan division of the vote, the pre-1970 model predicted the 1970 outcome to be 45.4 per cent of the vote for the party of the President, the Republicans; in fact, they won 45.7 per cent of the vote in 1970.

Table 4 shows the outcome of this historical

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[25] The discarding of observations one at a time coupled with re-estimation is the first step in producing a "jackknife" estimate of a complex statistic along with a confidence interval; see Frederick Mosteller and John W. Tukey, "Data Analysis, Including Statistics," in Gardner Lindzey and Elliot Aronson, eds., *The Handbook of Social Psychology,* 2nd ed. (Reading, Massachusetts: Addison-Wesley, 1968), pp. 133–160; and Rupert G. Miller, Jr., "The Jackknife—A Review," Technical Report No. 50 (August 28, 1973), Department of Statistics, Stanford University, Stanford, California.

[26] Polls reported in Gallup, *The Gallup Poll.*

**Table 4. Before-the-Fact Predictions of the Model**

| Model Based on Years | $\hat{\beta}_1$ | $\hat{\beta}_2$ | $R^2$ | Predicting Election of | Vote for President's Party: Predicted % | Vote for President's Party: Actual % | Model Absolute Error | Gallup Absolute Error* |
|---|---|---|---|---|---|---|---|---|
| 1938–1954 | .12 | .032 | .98 | 1958 | 46.0 | 43.9 | 2.1 | 0.9 |
| 1938–1958 | .11 | .032 | .85 | 1962 | 50.8 | 52.4 | 1.6 | 3.1 |
| 1938–1962 | .13 | .036 | .90 | 1966 | 51.9 | 51.3 | 0.6 | 1.2 |
| 1938–1966 | .13 | .035 | .90 | 1970 | 45.4 | 45.7 | 0.3 | 1.3 |
| 1938–1970 | .13 | .035 | .91 | 1974 | 39.2 | 41.1 | 1.9 | 1.1 |

* Average absolute error, Gallup = 1.5 percentage points.
Average absolute error, Model = 1.3 percentage points.

experiment for three other midterm elections—1958, 1962, and 1968. In each case, the predictions are based on a model estimated *prior* to the election predicted—that is, they are honest predictions. Note how stable the model is over the years, even though it is based on fewer and fewer elections as we go backwards in time. Table 4 shows that the model performs very well indeed in its predictions, doing even somewhat better than pre-election polls directly asking voters what party's candidate they intend to support in the upcoming midterm election. Table 4 thus provides a very strong test of an explanatory model based on non-experimental data—a test of predictive success. The before-the-fact predictive trials of the model show the following: in the four midterm elections from 1958 to 1970, the pre-election forecast generated by the model deviated by an average of 1.1 percentage points from the actual partisan division of the vote. In these tests, based on genuine prediction, the model performs successfully.

The model for the years 1938–1970 has performed well in all our statistical tests:

—high explanatory power, $R^2 = .91$
—statistical significance (.01 level) of all estimates
—independent replication, using other data or models, of parameter estimates
—no multicollinearity
—no outliers dominating estimates; stability of estimates when parts of data are discarded
—historical experiments: successful postdictions
—historical experiments: successful predictions.

This is all very nice, but how well did the model do in its predictions of the 1974 midterm congressional elections?

## The Model and the 1974 Elections: A Difficult Test in a Landslide Election

In the fall of 1973, I constructed Table 5—showing what the model would predict for the 1974 midterm, given varying levels of presidential approval ratings and performance of the economy. It was possible, then, to track the vote estimates for the huge changes in approval ratings and economic conditions as both President Nixon and the economy collapsed. Using data for approval rating and economic conditions, a pre-election prediction for the 1974 midterm was generated from the model. The calculations and the prediction were made public two weeks before

**Table 5. Predicted Republican Congressional Vote in 1974**

Predicted national congressional vote for Republicans in 1974 = $35.04 + .133\,P + .035\,\Delta E$,
where $P$ = per cent approving the job the President is doing
$\Delta E$ = yearly change in real disposable personal income per capita

| Percentage Approving the Job President is Doing ($P$) | Yearly Change in Real Disposable Personal Income Per Capita ($\Delta E$): −\$100 | −\$50 | \$0 | \$50 | \$100 |
|---|---|---|---|---|---|
| 25% | 34.9 | 36.6 | 38.4 | 40.1 | 41.9 |
| 30% | 35.5 | 37.3 | 39.0 | 40.8 | 42.7 |
| 35% | 36.2 | 37.9 | 39.7 | 41.5 | 43.2 |
| 40% | 36.9 | 38.6 | 40.4 | 42.1 | 43.9 |
| 45% | 37.5 | 39.3 | 41.0 | 42.8 | 44.5 |
| 50% | 38.2 | 39.9 | 41.7 | 43.4 | 45.2 |
| 55% | 38.9 | 40.6 | 42.4 | 44.1 | 45.9 |
| 60% | 39.5 | 41.3 | 43.0 | 44.8 | 46.5 |

the election; in addition, several other people used the model on their own to generate pre-election predictions. All in all, the election of 1974 provided a particularly stern test—both explanatory variables had undergone very large short-term shifts and had reached historical extremes in the period before the election. And Mr. Ford had only been in office for three months.

The prediction for the 1974 midterm was constructed from the model:

$$\begin{aligned}&\text{Standardized midterm loss in 1974 (predicted)}\\&\quad= \text{predicted Republican vote in 1974} - \text{average Republican congressional vote in prior elections, 1958–1972}\\&\quad= -11.083 + .133P + .035(\Delta E).\end{aligned}$$

The average Republican vote over the last eight congressional elections (from 1958 to 1972) is 46.12 per cent. Substituting this value into the above equation yields the estimates shown in Table 5. The predicted Republican share of the congressional vote in 1974 is shown for a variety of combinations of popularity levels and economic conditions and the exact prediction is determined by the approval level and economic conditions prevailing immediately before election. In the months before the 1974 elections, President Ford's popularity shifted greatly:[27]

| | |
|---|---|
| August 16 | 71% |
| September 6 | 66% |
| September 27 | 50% |
| October 22 | 55% |

The condition of the economy also changed greatly prior to the election; real disposable income in March, 1974 had declined about \$45 over the preceding year, but by the time of the November elections the decline was \$90. Putting the pre-election values for the approval rating and economic conditions into the equation led to the following late-October prediction for the 1974 midterm vote:

$$\begin{aligned}\text{Predicted Republican congressional vote}\\&= 35.04 + .133(55) + .035(-90)\\&= 39.2\%\end{aligned}$$

The final pre-election Gallup poll predicted 60 per cent Democratic, 40 per cent Republican; a national phone poll by Decision Making Information led to a 62–38 prediction; and our midterm model predicted 60.8 to 39.2[28] The actual vote was 58.9 to 41.1 in 1974.[29] Thus the error of the midterm model was 1.9 percentage points; of the Gallup poll, 1.1 points; and of the DMI poll, 3.1 points. Although the predictive error of the model for the 1974 election was slightly above its previous average, the model still performed very well in a most difficult election.

During 1973–74, both explanatory variables in the midterm model moved toward historical extremes, *both* high and low: presidential approval ratings ranged between 24 and 71 per cent; and $\Delta E$ between +\$198 and −\$90; and as these variables changed, the projected midterm vote shifted. These shifts can be compared with a series of polls taken during 1973–74 in which respondents were asked what party's candidate they intended to support in the upcoming congressional elections. Although the different polls themselves are not always consistent with one another, the projections of the midterm model follow rather well the shifts in the vote recorded in the polls (Table 6).[30] The dynamics of the midterm model show how economic conditions and the electorate's changing evaluations of Nixon and Ford shaped the Democratic landslide of 1974: while Ford's replacement of Nixon helped congressional Republicans by nearly six percentage points, most of that gain was offset by a declining economy and by the 15-point loss in Ford's approval rating following the pardon of Nixon.

Table 6 also indicates that the midterm model may over-respond to very extreme values in approval ratings and particularly in economic conditions; the model's projection computed in May 1973 deviates quite substantially from the Gallup poll (although less so from the Harris survey of the same month). Nevertheless the important point is not only that the model did survive the really difficult test of the 1974 midterm, but that it also helped assess the electoral effects of the

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[27] "Gallup Says Poll Shows Ford Popularity on Rise," *The New York Times*, October 24, 1974.

[28] "Poll Confirms GOP Fears," *The Washington Post*, November 4, 1974, p. A5.

[29] Vote as reported in *The Gallup Opinion Index*, 118 (April, 1975), p. 27.

[30] Data sources: Gallup approval rating for October, 1974 from "Gallup Says Poll Shows Ford Popularity on Rise," *The New York Times*, October 24, 1974; other months from *The Gallup Opinion Index*, 103 (January, 1974), 3; 108 (June, 1974), 1; and 111 (September, 1974), 12. The change in real disposable income per capita is available only by quarters and the monthly values are interpolated; it should also be noted that the quarterly figures are quite unstable, with provisional and final estimates often differing substantially. The computations are based on data in Bureau of Economic Analysis, *Business Conditions Digest*, January, 1975 (Washington, D.C.: U.S. Government Printing Office, 1975), p. 69. The reports of polls asking people how they intended to vote in the 1974 congressional election are from "Poll Confirms GOP Fears," *The Washington Post*, November 4, 1974, p. A-5; "Poll: Democrats Will Sweep Into the House," *New York Post*, August 8, 1974, p. 24; and *The Gallup Opinion Index*, 110 (August, 1974), 1–4. Nonresponses have been divided equally between the two parties.

Table 6. 1974 Election Projections from the Midterm Model and Polls, 1973–1974

| Date | Per Cent Approving President | Change in Real Income ($\Delta E$) | Projection of Midterm Model, % Democratic | Polls, % Democratic in Upcoming Election (G = Gallup, H = Harris, DMI = Decision-Making Information) |
|---|---|---|---|---|
| October, 1974 | 55% | −$90 | 61% | 60%, 62% (G, DMI) |
| July, 1974 | 24% | −$91 | 65% | 63% (H) |
| May, 1974 | 27% | −$78 | 64% | 60%, 64% (H, G) |
| March, 1974 | 26% | −$44 | 62% | 62% (H) |
| January, 1974 | 27% | +$22 | 60% | 59%, 65% (H, G) |
| October, 1973 | 28% | +$168 | 55% | 64% (G) |
| September, 1973 | 34% | +$168 | 54% | 58% (H) |
| May, 1973 | 44% | +$198 | 51% | 55%, 60% (H, G) |

political and economic earthquakes in the months prior to the election. And, of course, the experience of 1974 is clearly contrary to the textbook view of midterms—that off-year congressional elections are not much more than the electoral swing of the pendulum, mainly the consequence of differences in off-year compared to on-year turnout.

In summary, the tests of the midterm model confirm that even though the fitted equation is based on a relatively short series of elections, the quantitative results are quite secure—as indicated by the independent replications of both regression coefficients, the postdictive and predictive trials, the conventional tests of statistical significance, the pre-election predictions for the 1974 midterm, and the model's ability to move with changing events.[31] The midterm model also explains most of the variance in midterm outcomes. Few models of political behavior have passed such tests, particularly those of fairly complete statistical explanation, replication, and honest prediction.

We now consider the political significance of the midterm.

## From Votes to Seats

The political consequences of a midterm election flow from the resulting partisan distribution of seats in the House of Representatives, rather than from the partisan distribution of the national congressional vote. Since, as we shall see, the character of the translation of votes into seats has shifted greatly over the years, the political meaning of the midterm has itself shifted.

For midterms from 1938 to 1970, the relationship between seats and votes is a moderately strong one—variations in votes explain 76 per cent of the variation in seats. Much of the strength of that relationship comes from the more extreme outcomes of 1946 and 1958; omitting those years, the vote explains less than 4 per cent of the variance in seat shares. The lack of a really strong and consistent relationship between votes and seats in midterms is a reflection of changes in the swing ratio—the rates at which votes are translated into seats—over the years. The change in the value of the midterm vote in terms of seats can be seen by comparing the gain in the nationwide congressional vote made by the out-party with their gain in House seats. In the 1938 midterm, for example, the Republicans gained 7.7 percentage points in their national congressional vote compared to their 1936 congressional vote—and the result was an 18.1 per cent gain in their share of seats in the House. This yielded a swing ratio of:

$$\text{Swing ratio for 1938 midterm} = \frac{\%\ \text{change in seats, 1936 to 1938}}{\%\ \text{change in votes, 1936 to 1938}} = \frac{18.1}{7.7} = 2.4.$$

In the 1970 midterm, the Democrats gained 3.4 percentage points in their share of the congressional vote over 1968, but only 2.8 percentage points in seats:

$$\text{Swing ratio in 1970 midterm} = \frac{2.8}{3.4} = 0.8.$$

Thus a comparable change in the midterm vote in

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[31] The model appears to over-respond to very large short-run improvements in economic conditions. Such improvements have been more typical of on-years than off-years. The model performs acceptably for ups and downs of less than $150 per capita in real disposable income, which has been the case for all midterms since 1938. In adition, the model might be examined comparatively—for example, in British by-elections. Finally, the model could be extended for on-year elections (although Gallup did not ask the presidential approval question for incumbent presidents in the six months prior to on-year elections until the 1960s). On-year elections are clearly a much more complex problem; and the midterm model is best seen as an alternative to "surge and decline" and the similar approaches cited in footnotes 1–5.

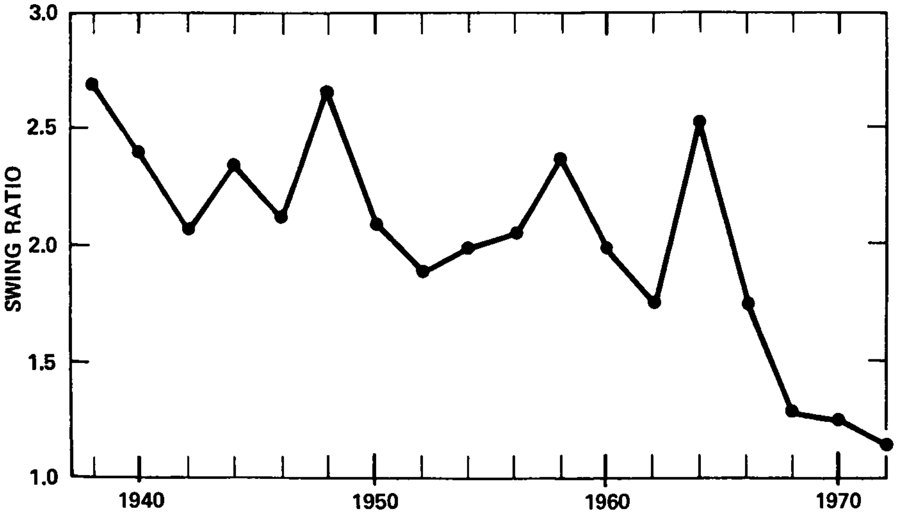

**Figure 3. Swing Ratio in Congressional Elections, 1938–1972***

* The swing ratio is the percentage change in congressional seats associated with a one per cent change in the nationwide congressional vote.

the 1970 midterm was worth one-third what it was in 1938. The trend in the swing ratio over all the midterms has been:

| | |
|---|---|
| 1938 | 2.4 |
| 1942 | 2.0 |
| 1946 | 2.0 |
| 1950 | 2.1 |
| 1954 | 1.6 |
| 1958 | 2.2 |
| 1962 | 0.2 |
| 1966 | 1.8 |
| 1970 | 0.8 |
| 1974 | 1.5 (approximately) |

Figure 3, recording the results of alternative estimates of the swing ratio, reveals the same pattern: a significant decline in the swing ratio over the years. The estimates of Figure 3 are computed as the least-squares slope calculated in the region within five percentage points of the actual nationwide congressional vote.[32] The estimates indicate that, in a normal election, shifts in the congressional vote are worth about half of what they were twenty years ago in terms of seats in the House of Representatives.

Figure 4 shows how the electoral systems of individual states have contributed to the nationwide change in the swing ratio in midterm congressional elections over the years. The graphs compare the translation of votes into seats for the midterm congressional elections of 1950 and 1970 in four large states. Note the difference between the seats-votes curves in the midterm of 1950 compared to 1970: a flat spot in the middle of the 1970 seats-votes curve has developed and, on that plateau, changes in the congressional vote in that state yield no changes at all in the partisan distribution of seats. Some of those flat spots are rather large; a party can gain ten or twelve per cent of the vote in every congressional district in a state and still not gain a single additional congressional seat. The swing ratio in such states is, for all practical purposes, zero: any change of ordinary magnitude in the vote results in no change in seats. The plateaus in the 1970 seats-votes curves are found in the region between 40 and 60 per cent of the vote for each party—right where the statewide congressional vote falls in most relatively competitive states. That is, elections are taking place on the section of the seats-votes curve that has the lowest swing ratio. The effect of all this is to secure the tenure of incumbent representatives, since they are invulnerable to vote swings occurring in typical congressional elections.[33]

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[32] This technique is described in Tufte, "Seats and Votes," pp. 549–551.

[33] The causes of recent increases in congressional tenure are not yet clear; see David R. Mayhew, "Congressional Elections: The Case of the Vanishing Marginals," *Polity,* 6 (Spring, 1974), 295–317; Walter Dean Burnham, "Communication," and Edward R. Tufte, "Communication," *American Political Science Review,* 68 (March, 1974), 207–213; and Robert S. Erikson, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," *American Political Science Review,* 66 (December, 1972), 1234–1255. Some consequences of seat changes are described in David W. Brady and Naomi B. Lynn, "Switched-Seat Congressional Districts: Their Effect on Party Voting and Public Policy," *American Journal of Political Science,* 17 (August, 1973), 528–543.

Thus the electoral system—the arrangements for the aggregation of the votes of citizens into seats in the House—does not respond consistently (and hardly responds at all in some states) to changes in the aggregate preferences of voters, even though the voters themselves are casting their ballots in a systematic and focused way in midterm congressional elections. No wonder midterm outcomes—especially when they were evaluated in terms of changes in seats rather than votes—appeared, in V.O. Key's words, as "a procedure whose strange consequences lack explanation in any theory that personifies the electorate as a rational god of vengeance and reward."[34]

## Conclusion

Our fundamental finding is that the vote cast in midterm congressional elections is a referendum on the performance of the President and his administration's management of the economy. Although the in-party's share of the nationwide congressional vote almost invariably declines in the midterm compared to the previous on-year election, the magnitude of that loss is substantially smaller if the President has a high level of popular approval, or if the economy is performing well, or both. The fitted model indicates that the aggregate midterm outcomes from 1938 to 1970 (omitting the wartime election of 1942) are explained by—and are predictable from—the economic condi-

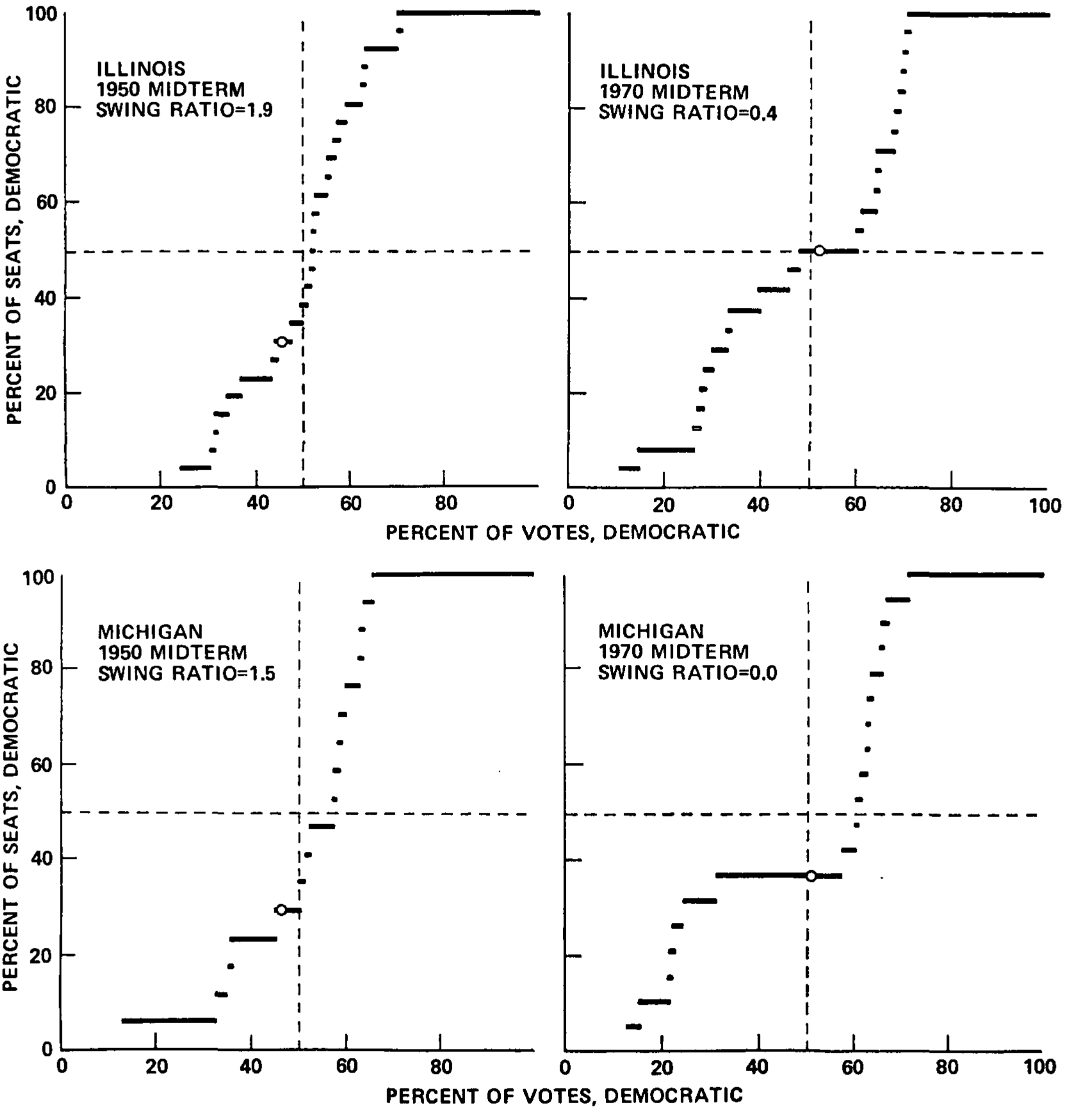

Figure 4. Votes-Seats Curves for the 1950 and 1970 Midterm Congressional Elections: Illinois, Michigan, Ohio, and Pennsylvania

tions and the level of approval of the President prevailing at the time of the election. To be specific: a change of ten percentage points in the President's approval rating in the Gallup poll is related to a change of 1.3 percentage points in the national midterm congressional vote for the President's political party; and a change of $50 in real disposable personal income per capita in the year of the election is related to a change of 1.8 percentage points in the vote. These estimates, although based on a relatively short series of elections, appear to be very stable and are confirmed by independent replication and by genuine predictive tests. From a statistical point of view, the model constitutes a virtually complete explanation of the aggregate vote in midterm elections: the model explains 91 per cent of the variation in the partisan division of the vote in midterms from 1938 to 1970; the model performed successfully in predicting the outcome of the 1974 congressional election.

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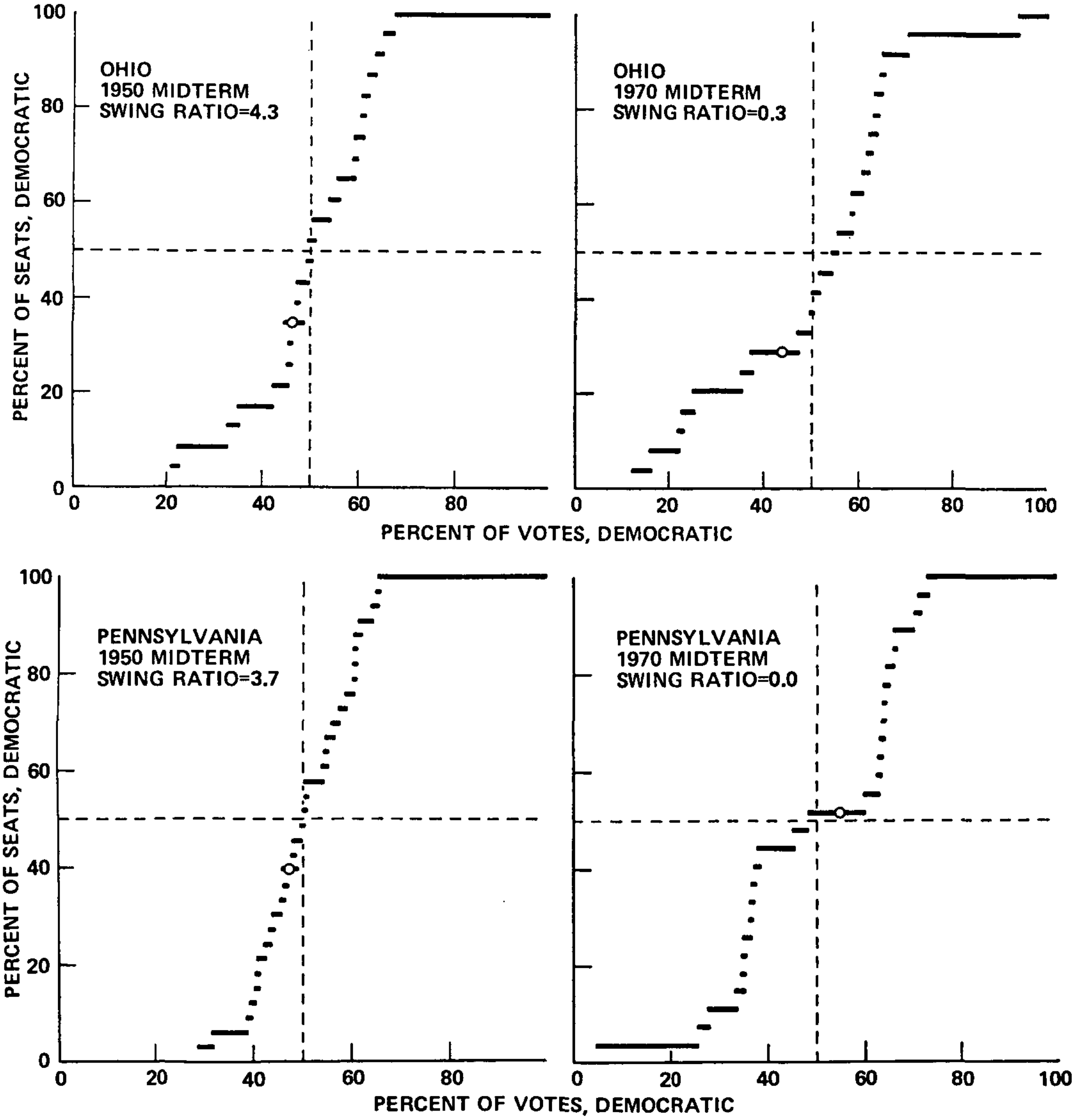
[34] Key, p. 568.

Our second main finding is that the midterm referendum of the nationwide congressional *vote* is often poorly reflected in the resulting partisan distribution of *seats* in the House of Representatives. Thus even though the voters, in aggregate at least, have done their best to make the midterm a referendum on the performance of the administration, their efforts are greatly muted by the structure of the electoral system.

The finding that the midterm vote does, in fact, constitute a referendum—albeit a sometimes hid-

**Figure 4. (Continued)**



den referendum because of shifts in the votes-seats translation—on the performance of the President and his administration's management of the economy is especially significant when compared to the political science textbook view of midterms. The standard view is, of course, that the midterm outcome derives from the prior on-year election, mostly a residual product of an electorate from which the short-term forces prevailing in the prior on-year have been subtracted. But our evidence indicates that the midterm is neither a mystery nor an automatic swing of the pendulum; the midterm vote is a referendum.

Along much more speculative lines, the midterm model also suggests a partial explanation of the fundamental fact of midterm elections, the loss of votes by the President's party. The model indicates that the loss occurs because the electorate's approval of the President has declined since the prior on-year election and because the economy is performing less well at the time of the midterm than it was two years earlier during the presidential election. Mueller has estimated the yearly decline of presidential approval at about six percentage points per year in office.[35] And, in general, the economy—measured here in terms of the yearly increase in real disposable income per capita—has historically performed better in on-years than in off-years.[36] At present, such an application of the midterm model is very speculative; I suspect that a satisfactory explanation of why the President's party always operates in the loss column in off-years will grow from a combination of the midterm model and a revised version of Campbell's "surge and decline" model (which, in revision, might place more emphasis on the surge and decline of coattail effects and less on turnout effects).

Let us conclude by considering the relevance of the findings for the eternal issue of voter rationality.

Stokes and Miller demonstrated that the midterm election could hardly be regarded as the electorate's evaluation of the legislative record of the two parties in Congress because an embarrassing number of voters lacked the minimal information required to cast a ballot informed by a judgment of a party's legislative performance. For example, a majority of those surveyed failed to recall which party controlled Congress. Less political information is demanded of voters, however, if they are to cast their midterm ballots as a referendum on the performance of the administration—only knowledge of what political party the President belongs to. And the link between the President's party and the congressional candidate is easy, because each congressional candidate's party is printed on the ballot along with the name of the candidate.[37] Thus the information necessary to cast an off-year congressional vote for or against the party of the President is at hand for most voters. If the information demands on voters are minimal enough (in this case, knowing the name of the President's political party), then the aggregate performance of the electorate can be consistent with objective factors prevailing at the time of the election.

The basic idea behind the model—that, in midterm congressional elections, at least some voters reward or punish the party of President by casting their votes for representatives in line with their perceptions and evaluations of the President and economy—is a theory about the behavior of individual voters. It is important to realize, however, that all we observe in these data is the totally aggregated outcome of the individual performances of the forty million voters who turn out in midterm elections. Many different models of the underlying electorate are consistent with electoral outcomes that are collectively rational; and the observation of aggregate rationality clearly does not imply a unique specification or description of individual voters or of groups of voters making up the electorate. Aggregate studies provide evidence about aggregates. And surely for many citizens of voting age the midterm is not a referendum on the performance of the incumbent administration: some do not have the opportunity for a referendum vote since they find only one party's candidate on their ballots; others rely entirely on party affiliation, name recognition, and incumbency to guide their decisions; and a majority do not show up at the polls in the off-year.

Consequently, our highly aggregated evidence speaks only most indirectly to the central political questions concerning the rationality of voters as individuals:

—What kinds of decision rules do individual voters use?[38] Which voters use what decision rules?
—What conditions encourage voter rationality?
—How may these conditions be nurtured?

These concerns once again emphasize the theoretical interest (as well as the continuing practical interest) in ticket splitters, swing voters, and citizens who fail to vote in some elections—for it is, after all, the aggregate combination of these individual effects that leads to the striking collective rationality apparent in our findings here.

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[35] Mueller, "Presidential Popularity," p. 25.

[36] Edward R. Tufte, "The Political Manipulation of the Economy: Influence of the Electoral Cycle on Macroeconomic Performance and Policy," manuscript, Princeton, 1974.

[37] Stokes and Miller, pp. 544–546.

[38] See Stanley Kelley, Jr. and Thad Mirer, "The Simple Act of Voting," *American Political Science Review*, 68 (June, 1974), 572–591.

# Underdevelopment and the "Gap" Theory of International Conflict*

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The diffusion of Western culture and economic patterns to other areas of the world has proceeded unabated for more than three centuries. Its forms, often violent and oppressive, have always been tempered by a faith in the ultimate benefits that the underdeveloped world would receive through the processes of cultural and economic interaction. While apologists for colonialism did not deny the strategic, prestige, and economic advantages accruing to the metropolitan states, they would point out simultaneously that the natives were also receiving benefits, including employment, stable government, Christianity, an end to local slave trade, and the introduction of education, democracy, and modern health facilities. Nevertheless, the objectives of most colonial regimes were limited to securing advantages for planters, traders, the military, and the church.[1]

During the past thirty years a greatly expanded view of the possibilities for the former colonial territories has developed. The ultimate fate of the underdeveloped societies is to become fully developed. A common view is that these territories will have to become replicas of the highly industrialized states of Western Europe, North America, and the Communist countries. There is no halfway station where a society can stop on the road of modernization. The "gap" between the rich and the poor is growing, and the only solution is presumably for the poor to catch up and to become basically what the rich are. The end state of development is not the African or Asian version of late nineteenth-century Sweden (economically speaking), but the fully modern industrial urban society of 1975 or 1995. To suggest anything less is to deny the value of equal welfare and to perpetuate two classes of international citizenship.

The rationale for the egalitarian view of world development is to some extent humanitarian: everyone in the world should enjoy the blessings of the advanced countries. In other cases, economic considerations—the growing interdependence of the world—underlie the imperative for "closing the gap." Another important consideration is international peace and security. Many statesmen, politicians, and academics have argued that neither domestic turmoil nor international wars can be avoided in a world of states many of whose populations suffer from poverty. The unrequited goals of the "revolution of rising expectations" in the underdeveloped countries will lead to frustration, domestic revolutions, political instability, and messianic politics. These are likely to have spillover effects into the international system, causing regional rivalries at best and the intervention of the great powers at worst.[2]

Another view emphasizes the "gap" between the rich and poor nations as a source of international conflict.[3] The powerful industrialized states constitute a model which the underdeveloped seek to emulate. Industrialization, mass consumption economies, modern military forces, high levels of technology, and welfare-oriented politics stand as the indicators of worth in today's world. To the extent that the underdeveloped countries are not achieving anything resembling parity with those who have established these standards, or, put in another way, to the extent that they do not receive adequate proportions of the "rewards" of the international system, frustration expressed through aggressive nationalism or adventurous foreign policies will result. Though few agree on the sources of the widening "gap" (neocolonialism and imperialism are the causes according to writers of the "New Left"; economic, social, and cultural "barriers" within the underdeveloped societies are the sources, according to more orthodox interpretations), there is a consensus that the condition left unresolved must lead inevitably to international conflict.

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[1] See, for examples, Richard D. Wolff, *The Economics of Colonialism: Britain and Kenya, 1870–1930* (New Haven: Yale University Press, 1974), and Ngo Vinh Long, *Before the Revolution* (Cambridge, Mass.: Massachusetts Institute of Technology Press, 1973).

[2] This common view is proposed, for example, in *Partners for Development: Report of the Commission on International Development*, Chairman: Lester B. Pearson (New York: Praeger, 1969), p. 7; see also Robert S. McNamara, *The Essence of Security* (New York: Harper and Row, 1968), pp. 145–146.

[3] See, for example, A. F. K. Organski, *World Politics*, 2nd ed. (New York: Knopf, 1968), and McNamara, p. 146; Angelos Angelopoulos, *The Third World and the Rich Countries* (New York: Praeger, 1972), p. 7. For a critique of the statistical biases and fallacies in measuring the "gap," see P. T. Bauer, *Dissent on Development* (London: Weidenfeld and Nicolson, 1971), chap. 1.

I shall subject this latter thesis to a critical scrutiny, not from the point of view of international conflict theory or a theory of the causes of underdevelopment, but from a perspective which emphasizes the impact of Western, including socialist, economic solutions on indigenous cultures and social structures.[4] From the perspective of future international relations, we can also inquire: If some underdeveloped countries adopt more nationalist economic policies, reject some forms of Western "penetration," and isolate themselves more from international cooperative ventures, will the Western response be hostility or accommodation? The phenomenon of isolation or quasi autarchy is seldom noted in the development literature, primarily, I suspect, because it challenges long-standing normative commitments to a world of increased international integration and communication. International relations scholars have also neglected the phenomenon because our ruling paradigm—the notion of an international system—does not lend itself to analysis of such normative problems as what *sorts* of influences (and what sorts of social consequences) permeate the "nodes," "networks," and "linkages" of the system. In brief, while not rejecting the "gap" theory of international conflict, I would like to explore an alternative scenario in which the international system becomes more fragmented and some underdeveloped countries selectively reduce their "interdependence."

The scenario derives from my critique of three components of the common Western image of underdevelopment. The first is the view that once having been influenced by modern communications, the people in the underdeveloped countries universally desire to change their life-styles and to adopt most, if not all, of the economic, cultural, technological advantages of the industrialized state. Thus, the main thrust for development comes from the grass-roots level, and local elites commit themselves to modernization in order to accommodate this "revolution of rising expectations." Second, our image of underdevelopment is based on considerable pictorial or written evidence of widespread poverty, despair, disease, lack of opportunity, and cultural deprivation. If the picture of underdevelopment in our heads includes these characteristics as typical, then of course the "revolution of rising expectations" seems a perfectly appropriate response. Third, given the first two components of the image, we often imagine that people in underdeveloped countries will quickly grasp the means by which their expectations can be met, readily accept the techniques necessary to lift themselves out of misery, and suffer few costs in doing so.

Many academic treatments of underdevelopment are more sophisticated than this description of these three components implies. Nevertheless, many fail to acknowledge the harmonious and satisfying aspects of the lives of nonindustrialized peoples, the costs involved in many development schemes, and the inappropriateness of some aspects of Western-style economic activity to other value systems. This shortcoming becomes apparent when one compares the writings of economists with those of many anthropologists. Economists, accustomed to thinking in terms of aggregates and quantitative indicators, necessarily make inferences about individuals from aggregate data. Most economists believe that people earning $200 annually must suffer from extreme deprivation in all social, physical, economic, and cultural dimensions. An anthropologist who has lived among people with such incomes, however, would see that that way of life typically includes close family ties, reasonable dietary standards (except in some countries such as Bangladesh, and during periods of natural catastrophe), cooperative work habits, effective, and often humane, social controls, and intellectual stimulation through acquiring knowledge of the natural environment and skills relating to that environment.

I will distort the picture in favor of the view of many anthropologists. This is done deliberately in order to highlight corresponding distortions in the more common view of underdevelopment, and so that we can begin to think critically about those conceptions of the future international system which are based on more "interdependence," more international trade, more private investment, and more urbanization and industrialization.

### Sources of the Commitment to Development

Given the strong resistance of many indigenous cultures to Western penetration[5] throughout the past three centuries, it is somewhat surprising that today we should accept uncritically the thesis which portrays tribal peoples and peasants as grasping uncritically for the benefits of modern Western society and technology. Most political leaders in the world have referred to the "revolution of rising expectations" as if this phenomenon were the sole source of the drive toward development. To be sure, few people reject all the benefits of modern technology, science, and economic activity. With a modicum of demonstration, most people can easily see the benefits of better health or of decreasing dependency upon the vagaries of nature. No longer does everyone accept misfortune, poor housing, clothing, and diet as the will of God, and many aspire to improve themselves and their children through individual and collective action. But we have often confused these relatively simple aspirations for a better life with the assumption that everyone wants to adopt *all* Western institutions through Western-type economic activity. We have confused development with outright emulation; a reasonable standard of living with large cities, heavy industry, mass consumption, personal mobility, rejection of agriculture as an appropriate "way of life," high per-capita income, personal ownership of an automobile, and the like.

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[4] Throughout this paper, the term "Western" includes most of the developed socialist states. The literature on the causes of underdevelopment is expanding rapidly, largely fostered by the pioneering work of André Gunder Frank. See particularly his essay "The Development of Underdevelopment," in *Latin America: Underdevelopment or Revolution?* ed. A. G. Frank (New York: Monthly Review Press, 1969). A summary and critique of the underdevelopment-dependency literature is in Benjamin J. Cohen, *The Question of Imperialism: The Political Economy of Dominance and Dependence* (New York: Basic Books, 1973).

[5] H. A. C. Cairns, *Prelude to Imperialism* (London: Routledge and Kegan Paul, 1965); Edmund Stillman and William Pfaff, *The Politics of Hysteria* (New York: Harper and Row, 1964); Ignacy Sachs, *La decouverte du tiers monde* (Paris: Flammarion, 1971), chap. 4.

Evidence that many people in underdeveloped countries do not accept all aspects of the "developed" society as an end state of human activity is not overwhelming but appears regularly. In Kenya, South Africa, Zambia, Sicily, and Thailand, for example, many go to cities and towns to work for wages, but a high proportion return eventually to the rural areas.[6] They want the wages that offer them some increased economic opportunities, but the major social, psychological, and emotional supports of their lives remain in rural areas.[7] Migration back to the country occurs despite the notable bias of economic development plans (Cuba, Taiwan, and Tanzania excepted) favoring urban over rural programs.[8]

This is not to deny strong grass-roots impetus for the betterment of life. But a case can also be made that the strongest thrust for modernization conceived as industrialization, urbanization, and consumerism derives from the aspirations of a relatively small, Westernized elite whose members have been educated and trained in the West or have pursued career opportunities which are closely linked to Western economic institutions and international organizations.

Another important source of the development thrust is national security. Nayar[9] shows that rapid industrialization in Japan, the Soviet Union, China, and India has related less to public welfare considerations than to the requirements of military security and national independence. The leaders of these countries, through their speeches and actions, have argued that modernization is a necessary condition for securing and maintaining independence in an international or regional system of hostile states. Even in some of the smaller underdeveloped societies, such as Thailand, Western economic techniques were borrowed as a means of reducing Western economic penetration.[10]

Closely related to the perception of external threat and the desire to secure national independence are considerations of prestige. Once the leaders of underdeveloped states become integrated into the international system, they adopt the measures of worth of that system.[11] Jacobs, for example, points out that a prime stimulus for economic modernization among the Thais is not the felt needs of the people, but the needs of the patrimonial leadership, which include the desire to be "politically respected in the halls of the developed Western powers."[12] Hence, as has been noted often in the literature, many visible indicators of modernity—commercial urban centers, national airlines, heavy industries, middle-class suburbs, and a plethora of private vehicles and consumer goods—are promoted less to assuage popular economic expectations than to bring diplomatic influence and prestige to the nation.

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[6] See, respectively, Colin Leys, "Politics in Kenya: The Development of a Peasant Society," *British Journal of Political Science,* 1 (July, 1971), 307–337; Sheila T. Van der Horst, "The Effects of Industrialization on Race Relations in South Africa," in *Industrialization and Race Relations: A Symposium,* ed. Guy Hunter (London: Oxford University Press, 1965), p. 112; Norman Long, *Social Change and the Individual* (Manchester: Manchester University Press, 1968), chap. 9; Johan Galtung, *Members of Two Worlds: A Development Study of Three Villages in Western Sicily* (New York: Columbia University Press, 1971); Norman Jacobs, *Modernization Without Development* (New York: Praeger, 1971), p. 178. Nevertheless, the growth of urban population in Africa, even if unstable, has been phenomenal. Most cities doubled their populations during the 1960s. For statistics, see Donald G. Morrison, Robert C. Mitchell, John N. Paden, and Hugh M. Stevenson, *Black Africa: A Comparative Handbook* (New York: The Free Press, 1972), part 2.

[7] The transfer of tribal customs and social relationships into an urban environment is possible. See Peter C. W. Gutkind, "African Urban Family Life and the Urban System," in *Urbanism, Urbanization, and Change,* ed. Paul Meadows and Ephraim Mazruchi (Reading, Mass.: Addison-Wesley, 1969), pp. 215–222.

[8] This bias is acknowledged in "International Bank for Reconstruction and Development," in *World Bank Operations: Sectoral Policies and Programs* (Baltimore: The Johns Hopkins University Press, 1972), p. 418. For a study of the antirural bias of development planners in India, see M. Lipton, "Urban Bias and Rural Planning in India" in *Development and Underdevelopment: The Third World Today,* ed. Henry Bernstein (Hammondsworth: Penguin, 1973), pp. 235–253; for Africa, see Gerald K. Heleiner, "Structural Change in Africa," in *The Widening Gap,* ed. Barbara Ward, J. D. Runnals and Lenore D'Anjou (New York: Columbia University Press, 1971), p. 92.

[9] Baldev Raj Nayar, "The Political Mainsprings of Economic Planning in the New Nations: The Modernization Imperative Versus Social Mobilization," paper presented at the conference "Asia in the Seventies," Carleton University, Ottawa, November 1971.

[10] Jacobs, *Modernization without Development,* pp. 131–133.

It is difficult to assess the relative weights of these sources of the drive for modernization. Undoubtedly they vary greatly from country to country, and in many instances large proportions of the population support their leaders' quest for international recognition and securing independence. Taken together, however, socialization, national security, and international prestige probably provide as plausible an explanation for the commitment to create a replica of the industrial states as does the hypothesis of a grass-roots revolution of rising expectations.

Some leaders, of course, do not see their nations as potential miniatures of Britain, the United States, or the Soviet Union. They have deliberately created alternative welfare models and pursued development strategies reasonably consistent with indigenous social and economic patterns. Julius Nyerere of Tanzania, for example, sees the goal of development as a reasonably independent economy based on local ownership, little direct dependence upon the major powers for capital or technical assistance, and a more or less egalitarian social structure in which the major segment, the peasants, work communally and cooperatively in accordance with, not contrary to, traditional social patterns.[13] Thai political leaders, despite their rhetoric in favor of economic rationalization and industrialization, maintain a firm commitment to political and economic forms which are based on patrimonial personal relations and which incorporate uniquely Thai ideas of moral goodness.[14] Other exceptions could be cited; but most leaders of underdeveloped states have adopted the Western (including variations of socialism) developed state as their goal or model, as well as Western analyses of their social and economic problems, and Western strategies for overcoming them.

Their goal is *economic growth.* United Nations reports indicate that although most governments also talk about social progress, equality, and advancement of culture, their budget allocations and administrative performances are geared primarily to a particular type of economic development.[15] These and other reports make it clear that the philosophy of Western-style economic development has spawned strategies favoring industry over agriculture, urban over rural areas, the modern over the traditional sector, and large capital-intensive projects over smaller labor-intensive initiatives. Income is distributed primarily toward urban middle classes, thus encouraging migration to cities and discouraging agricultural development.[16] The priorities of development policies thus seem to reflect short-run considerations of security and prestige, as well as a rather uncritical acceptance of Western definitions of development.

## Western Images of Underdevelopment: The Sources of Bias

There are frequently contradictions between popular Western images of underdevelopment (including assumptions about the pervasiveness of the revolution of rising expectations), and the social and economic realities some investigators find in underdeveloped societies.[17] Most striking is the strange conservatism among vast populations: many simply will not act according to Western conceptions of rational economic behavior. The resistance to urbanization, mechanization, capital savings, regular work schedules, achievement criteria, production of surplus, and to many other facets of Western commercial behavior is often noted in the literature, but less often taken seriously as evidence that some Western development goals and strategies are simply inappropriate for other value systems. The literature, moreover, makes no effort to link these resistances to foreign policy or to the overall character of relations between developed and underdeveloped countries. The "gap" theory of international conflict rests squarely on the notion of economic growth as the primary goal of underdeveloped societies and on the hypothesis that failure to reach the economic levels of the developed states will lead to frustration and aggressive nationalism. It is less often acknowledged that development itself may create strains and turmoil that will spill over into the international system.

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[11] Denis A. Goulet, *The Cruel Choice: A New Concept in the Study of Development* (New York: Atheneum, 1971), p. 80.

[12] Jacobs, p. 126.

[13] See the paper, "Ujamaa—The Basis of African Socialism," in Julius K. Nyerere, *Freedom and Unity: A Selection of Writings and Speeches* (London: Oxford University Press, 1967), pp. 162–171 and 183–188. The Arusha Declaration of 1967 explicitly rejects a Western consumption-oriented society for Tanzania.

[14] Jacobs, chap. 5, and pp. 314–316.

[15] Goulet, p. 70.

[16] *World Bank Operations,* pp. 96–97. Until the late 1960s, India's priorities lay clearly with heavy industry. Although 80 per cent of the population was employed in agriculture, only 20 per cent of public development expenditures went to this sector. See *Partners in Development,* pp. 287–288.

[17] Dan Usher describes finding that an early conception of living conditions among the Thais, based on economic studies, was greatly at odds with the reality he experienced living among them. The low per-capita income figures indicated virtually nothing about material welfare or sense of well-being. See his *The Price Mechanism and the Meaning of National Income Statistics* (Oxford: Oxford University Press, 1968), intro.

A number of reasons could be put forth to explain inconsistencies between images and realities. Given graphic details of the worst aspects of lives of people in underdeveloped societies—the street people of Calcutta or the *favelas* of Latin America—we assume that it is only natural for others to want what we have. We are seldom told, however, that often the worst social blights in the underdeveloped world are the *results of development policies* which place highest priority on urbanization, heavy industry, and "growth" in economic indicators, while simultaneously neglecting agricultural and rival problems.[18]

Others have looked at the problem of underdevelopment primarily in terms of the Cold War and have seen only two paths or models of development—socialism and free enterprise. The possibility of some entirely new strategies for development, or goals which realistically fall far below a per capita income of $800, are simply not considered; an economy based on farming, fishing, handicrafts, and some light industry and commerce just does not add up to a socialist or Western image of being "developed."

The main reason for failure to understand indigenous resistance to development may be, however, common Marxist and liberal assumptions about the worth of economic activity. Development analysts for the most part carry these assumptions into their images of the underdeveloped world, into the goals they project, and into the strategies they propose for achieving those goals. We need to go back at least to Bentham to find the intellectual source of our contemporary preoccupation with economic growth. His simple assumption was that the maximization of wealth led to the maximization of happiness ("each portion of wealth has a corresponding portion of happiness . . . "). Not only does Bentham describe the basically appetitive nature of man in economic terms, but he is among the first to argue that acquisitiveness is both rational and morally commendable in its consequences for private and public welfare. His view of the economic mainsprings of human activity still characterizes Western images of development. It is his version of "economic man" which most developers are trying to mold or create; they cannot see how people who do not share Bentham's view of human motivations can be rational or happy. The prime prerequisite for aid programs is thus to instill a respect for materialism and to overcome all those aspects of a culture which impede economic activity.[19] Economic sectors composed of producers who meet only immediate needs are labelled "stagnant." If there is no economic "growth," there can be no cultural progress.[20]

Take one example. Norman Jacobs presents an interesting analysis of modernization and development in Thailand in which he laments the fact that values other than economic maximization are rampant in the society. While Jacobs cannot entirely hide an admiration for the Thais' proverbial propensity to enjoy life—even to the point where they would rather go to a party or ceremony than show up for work—his overall conclusion rests squarely in the tradition of Western writing on underdevelopment: no matter how one may enjoy life through noneconomic activities, it is better to work hard and create institutions which encourage savings, long-run investment, production beyond immediate needs, strict work schedules, and the like.

Now if development (defined by Jacobs as "maximizing the economic potential of the environment")[21] is the most important personal and national goal, then of course conventional analyses of the problem of underdevelopment may be valuable. But many discussions of development fail to demonstrate that economic maximization is a superior value for the individual, even if it is important at the aggregate level. In some societies, of course, overpopulation forces governments to act *as if* production were the first national and individual priority. In those areas where there is still considerable land available and where some people prefer not to adopt Western-style value structures, should they be condemned or urged to act to the contrary? The unwillingness to save, to increase output, or come to work according to a rigid schedule should indicate something about the preferences of people about their life-styles, rather than being portrayed as "irrational" behavior, the consequences of superstition, or plain stubbornness.

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[18] For an account of the depressing effects of unbalanced urban industrialization on a rural population, and the distortion of government expenditures in favor of the modern sectors, see Robert H. Bates, "The Policy Origins of Migration in Zambia," paper presented at the meeting of the American Political Science Association, New Orleans, September, 1973.

[19] Among the many examples in the literature, see the attitudes expressed implicitly and explicitly in George M. Guthrie, *The Psychology of Modernization in the Rural Philippines* (Quezon City: Ateneo de Manila University Press, 1970).

[20] Virtually all commentators characterize nonindustrializing and non-high-growth economies as "stagnant." For one example, see E. K. Fisk, *The Political Economy of Fiji* (Canberra: Australian National University Press, 1970). Fisk praises the Indian community in Fiji for its aggressive economic advancement, while criticizing native Fijians for their lack of interest in large-scale economic pursuits.

[21] Jacobs, *Modernization Without Development*, chap. 5.

But, it can be argued, most of the people we are discussing live in such pitiful conditions that they must be shown how to become economic maximizers. If we go to the extreme of cultural relativism and accept *all* traditional practices as "functional," we are overlooking the problem of some objective minimum standard of general welfare. Growing populations, urbanization, and the requirements of international politics do not allow us the luxury of respecting traditional values and pursuits. Modernization can only come about through breaking up traditional social and economic patterns.

There is merit in this reply, particularly if we agree that in all cases population pressures allow no alternatives, and uncritically accept the assumption that a precarious economic existence must cause intellectual and cultural poverty as well. These positions may be substantially correct in countries such as India, Egypt, Pakistan, and in regions subject to natural disasters. Even in moderately developed countries such as Mexico, recent studies describe rural life in a manner to substantiate Hobbes's characterization of man in the state of nature.[22] Oscar Lewis's scenarios show a life with no reprieve from economic insecurity, a life of constant fear and distrust of friends and neighbors and of virtually no opportunities for self-betterment.[23] In southern Italy and Sicily, too, peasant life is known for "La Miseria." Edward Banfield's classic study, *The Moral Basis of a Backward Society*,[24] leaves little doubt that some sort of economic development is essential if the people of the region are to enjoy even a modicum of opportunity and a diminution of distrust, suspicion, and miserliness.

Yet when we realize that many of these studies portray societies undergoing rapid economic and social change, or describe characteristics that are also prevalent in developed societies (gossip, insecurity, and the like), then other portrayals take on more credibility. Robert Redfield, after years of field research in Mexican peasant communities, came to appreciate many aspects of peoples' lives in an underdeveloped community.[25] He not only pointed to the integrative and satisfying aspects of small community life, but implied that mental balance cannot be sustained in a society undergoing rapid change. While Redfield was not opposed to change, he saw that raising expectations which cannot be fulfilled only creates frustration, and that a great deal of so-called development creates wants "whose satisfaction brings no satisfaction."[26] More important, "[the peasants'] way of life, the persisting order and depth of their simple experiences, continue to make something humanly and intellectually acceptable of the world around them."[27] More contemporary observers of peasant communities are similarly impressed by, and respectful of, traditional cultures, and sensitive to the problems created by technological innovation, industrial development schemes, and the general penetration of relatively isolated communities by Western values and mores.[28] Redfield's observations are by no means exceptional. Thus, the wretchedness of most underdeveloped peoples' lives should be viewed as a hypothesis, not as a universal fact. At the very least, the first component of the common Western image—widespread economic and intellectual poverty—needs to be spelled out in much greater detail. The urban slum-dweller of Bogotá cannot be equated with the South Pacific islander or the Thai subsistence farmer. Per capita income figures reveal little about happiness or misery among such diverse people, much less about the importance of economic activity to their welfare. Indeed, the whole notion of "development" needs to be broadened to include more profound objectives such as esteem, self-actualization,[29] justice, and the maintenance of emotional communities. Economic growth may have only slight impact on these forms of development, and in some cases it is actually inconsistent with them. A brief review of some of the nonfinancial costs of economic development policies will help explain why resistance to development exists, and why the "revolution of rising expectations" provides a questionable basis for the theory that international conflict will result from the "gap" between developed and underdeveloped countries.

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[22] For a summary of the debate on characteristics of peasant societies, see George Foster, "Interpersonal Relations in Peasant Society," in *The Human Factor in Political Development,* ed. Monte Palmer (Boston: Ginn, 1970).

[23] Oscar Lewis, *Five Families: Mexican Case Studies in the Culture of Poverty* (New York: Basic Books, 1959).

[24] (Glencoe, Ill.: The Free Press, 1958).

[25] *Chan Kom, A Maya Village* (Washington: Carnegie Institution of Washington, 1934); *Tepoztlán: A Mexican Village* (Chicago: University of Chicago Press, 1930); and *The Little Community, and Peasant Society and Culture* (Chicago: University of Chicago Press, 1960).

[26] *The Little Community,* p. 63.

[27] Redfield, *Peasant Society and Culture* (Chicago: University of Chicago Press, 1956), pp. 132–133.

[28] For others, see John Greenway, *Down Among The Wild Men* (Boston: Little, Brown, 1972); Aliczia Iwanska, *Purgatory and Utopia* (Cambridge, Mass.: Shenkman, 1971); Lisa Peattie, *The View from the Barrio* (Ann Arbor: University of Michigan Press, 1968); Colin M. Turnbull, *Tradition and Change in African Tribal Life* (Cleveland: World Publishing Co., 1966).

[29] Goulet, *The Cruel Choice,* chap. 3.

### The Costs of Development

George Foster's analysis of underdevelopment and his solutions to it are reasonably typical of the literature. The requirements for inducing development include reducing the strength of conservative forces or neutralizing their results, while simultaneously strengthening the forces of change.[30] How is this to be done? Among other things, all cultural and social "barriers" to development must be overcome. These include "the basic values of [a] group, its conception of right and wrong . . . [and] the 'fundamental fit' or integration of its parts. . . . "[31] Barriers such as the social structure of groups and the prevailing type of family relationships must also be amended so that economically maximizing behavior can result. In short, social and cultural patterns which are incompatible with industrialization, "community development," and many other favored Western notions of what constitutes a good society, must be eradicated. While Foster does not endorse the "stern measures" advocated and practiced by developers in the socialist countries,[32] it is not hard to see why people who have any pride in or attachment to their culture, folkways, and social patterns might react negatively to such prescriptions for "growth."

One of the foremost analysts of underdevelopment, Gunnar Myrdal, appears no less impatient of those "barriers" to systematic national planning and Western-style economic activity and institutions. His major work in the field, *Asian Drama: An Inquiry into the Poverty of Nations* (1968), is replete with disparaging references to language and ethnic differences among peoples, to religious traditions and customs which go back several thousand years, and to other social institutions which seem incongruent or incompatible with drastic land reform, heavy industrialization, and above all, with nationwide planning programs that move people in the same manner that one moves nonhuman resources. Myrdal is not reticent in urging compulsion where growth is inhibited by noneconomically maximizing behavior —in short, he believes man should be forced to become more wealthy.[33]

Most economists and anthropologists note that there are costs to be paid in programs which involve breaking down traditional social patterns and practices. But they seldom dwell on these costs, and very few indeed have acknowledged that certain forms of development are not worth the price they extract.

Consider some of the development costs listed by Foster and others.

(1) Destruction of the "extended family." By breaking down traditional family patterns which emphasize mutual obligation, a productive man is liberated so that he can become part of a mobile labor force. He casts off his family obligations (presumably to be taken over by the state) in order to maximize his economic and, perhaps, career possibilities. The destruction of the extended family is directly attributable to the demands and opportunities of modern economic organization and of technology.[34]

(2) Destruction of traditional rural cooperative work patterns. Although communal work may not be as productive as individual enterprise, it fulfills social needs that cannot be met through private labor. The introduction of cash crops and money are directly linked to the demise of communal work in rural areas.

(3) As peasants become integrated into a commercial economy, dietary standards often decline. According to various studies cited by Foster,[35] a significant proportion of malnutrition in the world can be attributed to the substitution of packaged foods for traditional dietary sources. People purchase packaged foods primarily for their prestige value, the efficacy of advertising, or aesthetic rather than dietary reasons. (This phenomenon raises the question of measuring primary need satisfaction. Most development planners would identify increased cash spent on food as indicating growth and progress, whereas from a dietary point of view, needs might be met less adequately than previously. The availability of packaged foods, like many other commodities, is also linked to escalating definitions of primary needs among populations; once obtained, they tend to become necessities.) Another study discloses that many mothers in agrarian societies, imitating middle-class patterns in the West, have given up breast feeding their babies, with resulting undernourishment and protein deficiency.[36]

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[30] George Foster, *Traditional Cultures and the Impact of Technological Change* (New York: Harper, 1962), p. 59.

[31] *Ibid.*

[32] The ethnocentrism of analysts of underdevelopment in the socialist countries is remarkably explicit. All local customs which inhibit industrialization and the development of an urban proletariat must be destroyed, no matter at what cost. There is only one route to modernization, namely heavy industry and collective agriculture, based on the Soviet pattern. V. G. Solodovnikov, *Afrika vybirat put* (Moscow: Hayka, 1970).

[33] For a critique of Myrdal's approach to economic development, see Bauer, *Dissent on Development*, chap. 5.

[34] Foster, *Traditional Cultures . . .*, pp. 30–40; Hafeez M. Said, *The Village Culture in Transition* (Honolulu: East-West Center Press, 1970), pp. 140–143.

[35] Foster, *Traditional Cultures . . .* , pp. 33–37.

[36] An FAO study cited in Conrad M. Arensberg and Arthur H. Niehoff, *Introducing Social Change* (Chicago: Aldine, 1971), p. 126.

Many other costs could be cited. Perhaps chief among them is the rise of class distinction. In villages which are patterned on communal labor and mutual help, introduction of larger incomes and the availability of Western-style consumer goods creates status distinctions that are disruptive to cooperation, political decision making, and to inter- and intrafamily harmony.[37]

But what is most deficient about the notion of economic "growth" as measured by increased per capita income is that it reflects little about the sense of well-being. Various studies show that there is no apparent connection between the massing of consumer goods—even home comforts—and happiness. Development breeds new wants and the perception of new needs, but when these have been met, the people do not feel they are better off.[38] At the same time, one investigator, reflecting his American values, was surprised to find a great amount of contentment among rural Filipinos living at subsistence or near subsistence levels.[39] Such contentment did not in the least suggest to the researcher that perhaps the people in question should be left "underdeveloped."

Finally, there are the indicators of social stress resulting from rapid cultural change. These do not appear everywhere, of course, but the question of people's ability to undergo change involving destruction of traditional economic and social patterns is not yet understood well enough. (Even in the United States, government officials have resisted the idea of an annual "social state of the union" report. The traditional "State of the Union message" is concerned mostly with economics, as if all other "states" depended upon the rise and fall of economic indicators.) Consider the following report upon the consequences of a well-intentioned Danish program to offer training, economic opportunities, and the cultural benefits of town life to native Greenlanders: According to a Greenlander member of the Danish parliament, many benefits have resulted from these programs, "but we young Greenlanders find that the creation of a modern social pattern has not been accompanied by human growth, mental stability, psychological balance—in short, human well-being—in the new environment. Families are breaking up, crime is increasing, alcoholism is spreading, even to the very young. The number of psychological disturbances is increasing, also among school children. Many people discern an inner confusion and disillusion. The social privation of the past is being replaced by human disintegration."[40] Similar observations have been made about Fiji, the Philippines, and the aborigines of Australia.[41]

Recognition of these and other costs have not, for the most part, reduced the zeal with which many governments and some populations pursue the road to modernization. Apparently most have agreed that on balance the costs are worth the advantages, or, without looking around for alternative images or strategies of development, they have assumed that the degree of need allows no other course of action. Most, however, simply do not make an assessment of the relative balance between the costs and advantages. The latter are assumed to be obvious and preponderant. Yet, we would expect that as some of the costs become more obvious and painful, some governments, supported by people facing cultural stress, will begin to acknowledge the political benefits of resisting further westernization. It is then that we could expect to see more "buffers" erected by the governments of underdeveloped states against the penetration of foreign influences.

### The "Maldeveloped" Societies

Most of the literature on development assumes that the paths we have trod must serve for others as well, give or take a few national peculiarities.[42] But it is becoming increasingly apparent that as our own growth continues at astonishing speed, increases in indicators of social stress are occurring with disturbing regularity. The British government's recent "official social commentary" on the state of England shows impressive economic gains over the past decade but reveals that in the same period violent crimes have almost trebled and that many other indicators of serious social problems are rising at a rate much faster than population growth. The number of women employed part- or full-time in the world's oldest profession has increased at an alarming rate in Italy and Israel, while in the United States, murder, assault, and burglary rates have doubled in most cities during the past decade. The number of people hospitalized for mental illness is growing porportionately much more rapidly than is the general population—one-half of all beds in American non-Federal hospitals are occupied by people suffering from mental disorders[43]—while child beatings are reported to be increasing dramatically. Alarmed physicians note that child beating usually serves the needs of the attacking adult seeking relief from uncontrollable anger and stress.[44] For their part, the young in many countries are contributing to an ever-swelling juvenile delinquency rate. While all these phenomena cannot be attributed to a single cause such as social change or urban density, it is significant that their highest incidence is commonly found in urban areas.[45]

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[37] For other costs, see Herbert Blumer, "Industrialization and Race Relations," in *Industrialization and Race Relations,* ed. Hunter, p. 226; V. H. Joshi, *Economic Development and Social Change in a South Gujarat Village* (Baroda: University of Baroda Press, 1966), pp. 101–113; and James M. McKendry et al., *The Psychological Impact of Social Change in the Philippines* (Science Park, Penn.: H. R. B. Singer, Technical Report 857-R-2, n.d.), pp. 66, 72.

[38] Goulet, *The Cruel Choice,* p. 80; Redfield, *The Little Community,* p. 62. Quantitative evidence supporting this statement is found in Guthrie, *The Psychology of Modernization . . . ,* pp. 90, 96.

[39] McKendry et al., p. xii.

[40] George Boultwood, "Danish Plan for Greenland Too Fast," *The Montreal Star,* October 12, 1972.

[41] Statement of Fiji minister of tourism to a meeting in Djakarta, reported in *The Sun* (Vancouver), Sept. 4, 1974, p. 34; "Civilization Brings Grief to Aborigines," *The Sun* (Vancouver), Sept. 19, 1974, p. 42; McKendry et al., p. 18.

[42] Possibly this assumption is being questioned more frequently today. See, for example, the testimony of U. Alexis Johnson, former Under-Secretary of State in the United States, before the House Subcommittee on National Security Policy and Scientific Development. Johnson claimed that the concept of development needed to be redefined to eliminate the worst effects of industrialization as experienced in the advanced countries. U.S., House of Representatives, Committee on Foreign Affairs, Subcommittee on National Security Policy and Scientific Development, *National Security and Changing World Power Alignment,* 92d Cong., 2d Sess., 1972, pp. 362–394.

It may be that the worst *physical* aspects of rapid industrialization, so precisely described by Dickens and Zola, are behind us. What may lie ahead of us is social stress and possible breakdown. The other manifestations of the maldeveloped society—pollution, destruction of wilderness, waste, depletion of resources, and the like—are too well-known to require further comment. In the face of these physical and social consequences of rapid change, we must begin to wonder if our prescriptions for others are really well-advised. The model of the "developed" state seems increasingly tarnished. When elites or others already subject to the strains caused by development begin to learn of our difficulties, they may well question whether they are willing to undergo similar consequences. Disillusionment with the "developed" model may thus become another source of policies which attempt to isolate underdeveloped areas from certain forms of Western penetration.

## Avoiding Conflict between Underdeveloped and "Maldeveloped" States: Some Solutions to the "Gap" Problem

The Pearson Report, government statements in support of private investment and foreign aid, and academic treatises as well agree basically with the view expressed by former Under-Secretary of State U. Alexis Johnson in 1972 that the growing gap between the rich and poor is a "fundamentally unacceptable situation [which is] explosive in a political sense."[46] The solutions offered to this problem have been mostly continuations and expansions of past approaches: more trade, lower tariffs in the industrial countries, more private investment, and more international cooperation. For Mr. Johnson, the solution is greater international cooperation and increased interdependence. The "foundation bricks" of his image of a more egalitarian world order are being laid by private citizens, and particularly by private businesses and multinational corporations. The gap will be closed to the extent that these private networks of association grow. While Mr. Johnson is expressing a private opinion, his view is consistent in its fundamentals with the solutions proposed by many governments and international agencies.

Another view, expanded at length by Denis Goulet, argues that the gap can never be closed until the developed states allow the other areas of the world to share in their decision-making power and wealth. All the exploitative aspects of contemporary international economic relations must be ended, and the small states suffering from underdevelopment, status inconsistency, or whatever, must be made into active partners in the formulation of world monetary, trade, and aid policies. But the main ingredient of the solution must be a willingness by the "maldeveloped" states to *decrease* their levels of consumption in order that others may have more. There must be a fundamental redistribution of political power and economic resources in the world.

Mr. Johnson's solution contains several immediate problems. It assumes, first, the availability of infinite resources in the future. While Johnson may recognize some of the shortcomings of advanced societies, his plea for more interdependence—more trade, private investment, tourism, and extraction of natural resources—does not reveal how it attacks such fundamental problems as overpopulation and the present distribution of energy use. Edward Woodhouse has argued recently that given finite resources, we can no longer adhere to the view that economic development should be the primary goal of all states, or that "unlimited urbanization, industrialization, and GNP growth . . . are in fact possible or worthy any longer."[47] In his opinion, development as it has been defined by most economists is a chimera. The dwindling availability of resources, in addition to the pollution problem, simply will not allow the world to be composed of 140 or more highly industrialized collectivities. More "interdependence" as proposed by Mr. Johnson sounds as if there should simply be more "growth" in all economic dimensions.

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[43] Noted in Robert C. Wood, "The Future of Modernization," in *Modernization,* ed. Myron Weiner (New York: Basic Books, 1966), p. 49.

[44] *The Globe and Mail* (Toronto), August 18, 1971, p. 8. The rising figures on child abuse may be accounted for, on the other hand, by new legislation requiring physicians to report incidents to police authorities.

[45] A study by W.M.S. and Claire Russell demonstrates the positive correlation between urban density and crime rates, reported in *The New York Times,* August 16, 1970, sec. 1, p. 53. Care must be taken in interpreting crime and mental health statistics, as these may be manipulated for political purposes, or they may reflect better detection methods.

[46] *National Security and Changing World Power Alignment,* Johnson's testimony on August 7.

There is little reason to believe, moreover, that the governments of underdeveloped countries will continue to accept uncritically those relationships which Western governments and multinational enterprises define as "interdependent." To an increasing number of observers, these relationships are being defined not as "cooperation," but as dependence (at best) or imperialism (at worst). The continuation of present economic trends in the world is not likely to allay these fears; quite the contrary. More trade, more aid, more private investment, more tourism, and more private contacts—the traditional ingredients of the liberal view of internationalism—are all implied in Johnson's formula, but he fails to take into account that the consequences of these types of interactions often create feelings of resentment, racial antagonism, and perceptions of exploitation.

The solution offered by Goulet seems attractive. It acknowledges the problems of dwindling resources, and unequal decision-making power and economic rewards in the international system. But there is little evidence that any government in a "maldeveloped" state is even beginning to think in his terms. While many governments have begun to organize anti-pollution programs, none has yet instituted a regime of rationing in order to reduce pollution or to save resources . . . even for its own population. That it would do so to redistribute the world's resources in favor of the underdeveloped states seems highly unlikely in the foreseeable future.

## The Underdeveloped States Reject the "Maldeveloped" States: An Alternative View of the Source of International Conflict

Although academic researchers show little agreement on the correlation between the state of the underdevelopment and the propensity to engage in aggressive foreign policy,[48] we cannot dismiss entirely the "gap" theory of international conflict. There might well be more military crises triggered by domestic turmoil in the underdeveloped countries, and the "have nots" may, as revealed in the Arab oil embargo of 1973, undertake aggressive policies toward the "haves."

Many governments, of course, have probably gone too far down the path of industrialization, urbanization, and consumerism to change their course significantly. Many will continue to pursue Western-type development for reasons of national security or international prestige, or because their westernized elites (including Marxists) remain cut off from their roots. Most of the Latin American countries are already heavily urbanized and, despite large population increases, have shown impressive economic growth rates. India, Egypt, and some of the other more overpopulated countries will probably continue industrialization programs enabling them to maintain regional leadership roles, prevent overdependence on great powers for military supplies, or avoid the consequences of not having adequate arable land to sustain rapidly growing populations in rural areas. The alternative scenario, then, would refer primarily to some of the smaller states in Africa, Southeast Asia, the South Pacific, and possibly the Middle East. These are the states already on the periphery of the international system, least touched by Western influences and thus perhaps most vulnerable to cultural dislocation caused by increased penetration from outside.

These states, rather than accepting greater interdependence, would seek to reduce the penetration of external societies. Tourism would be discouraged; foreign aid turned down; aid experts expelled; foreign corporations nationalized; and new multinational enterprises denied opportunities for exploitation of natural resources or development of markets. While not all forms of contact would cease—trade, normal diplomatic contacts, and some international cooperative programs would remain—those which were identified as "endangering" the local culture or creating undue dependency on outside states would be curtailed or terminated. The models of Burma under General Ne Win and Sikkim until recently come to mind.

What sorts of evidence might be put forth to support even a minimal confidence in the scenario as a possibility? There is no systematic evidence yet, much less an identifiable trend, but some indicators of the rejection of liberal nostrums for the "gap" problem are beginning to appear. First, at the grass-roots level, there is the persistence of millennial movements in many African and some South Pacific societies. Lanternari[49] shows that almost all of these movements and their early twentieth-century predecessors are a response to extreme external disturbances to indigenous ways of life. The people seek relief from their frustrations and sufferings in religious ways, usually before they turn to political means. It is thus possible to speculate that many of the adherents of millennial movements today might someday constitute a fertile ground for strongly anti-Western political leaders. The religious prophets of these movements could also adopt political roles.[50]

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[47] Edward J. Woodhouse, "Re-visioning the Future of the Third World: An Ecological Perspective on Development," *World Politics*, 25 (October, 1972), 9.

[48] For example, Rudolph J. Rummel, "The Relationship Between National Attributes and Foreign Conflict Behavior," in *Quantitative International Politics: Insights and Evidence*, ed. J. David Singer (New York: The Free Press, 1968), pp. 187–214. See, however, the chapter by Michael Haas in the same volume.

[49] Vittorio Lanternari, *The Religions of the Oppressed: A Study of Modern Messianic Cults* (New York: Knopf, 1963).

Second, most of the current generation of leaders in the underdeveloped countries are highly "westernized." But what of a new generation of leaders, some of whom will be less personally familiar with the West and whose careers might be founded on a populist base that contains anti-Western attitudes? Is President Amin of Uganda —a man whose career includes few of the experiences his African counterparts had in Western societies—an exception, or might others of his persuasion and tactics follow? If nativist political groups begin to flourish, will the external sources of the commitment to development—prestige, security, and socialization patterns—remain so potent?

It has been argued, third, that part of Allende's appeal in the 1970 elections in Chile was based on his handling of the issue of foreign influence in the Chilean economy. Aside from the well-known American corporations operating in the country, there was a rapid growth of foreign ownership of Chilean manufacturing in the late 1960s.[51] While the Chileans were by no means rejecting Western patterns of economic organization in general, the popularity of appeals against foreign economic penetration was undeniable. Chile will probably not be the last country which decides to accept the costs of decreasing foreign economic influence in order to enhance governmental control over the direction of development.

Fourth, the notion of mutual benefits accruing to underdeveloped countries through capital transfers and the operations of foreign-based corporations is being increasingly scrutinized. Except among Marxists, it has been commonly assumed until recently that private investment from the industrialized countries brings significant economic gains to the host underdeveloped countries. The transfer of technology and of managerial skills, along with the training of a skilled work force, is a positive consequence of this activity. And a good case can be made that foreign investment and production which lead to import substitution help the host country's balance of payments position.[52] Yet there are also tangible and intangible costs to the host country. Some studies show, for example, that over the long run, the flow of capital tends to reverse itself; remitted profits, royalties, and management fees become greater than the amount of new capital flowing into the host country. In the period 1950–1965, remittances of Latin American subsidiaries of American parent companies exceeded new private investment by $7.5 billion, and for the period 1965–1969 the gap was an additional $3 billion.[53] When we consider that many subsidiaries of multinational corporations provide their own managerial talent, that they import components from parent companies (rather than purchase them locally), and that in many ways they reduce the possibilities for local firms to survive,[54] the benefits accruing to the host country begin to pale. Even employment benefits may be much less than is often assumed. Capital-intensive projects simply do not provide the opportunities for employment that, let us say, investment in agriculture would.[55]

Moreover, most foreign-owned manufacturing enterprises in underdeveloped countries must work assiduously to shape Western-style consumption habits among people becoming part of the market economy. Yet it is precisely these habits which are inimical to many preferred development strategies. People are encouraged to acquire tastes which, while perhaps meeting prestige needs, make little contribution to the general welfare or to community development. As Illich has pointed out, every automobile manufactured and sold in Latin America implies that more rational mass-transportation policies are less likely to be developed. As cars are purchased, national resources will have to be (and have been) devoted to the construction of mass highway systems, benefiting primarily the modern urban middle class. Correspondingly less will be spent, we can assume, on high capacity systems of urban and rural transportation. Every private refrigerator sold makes it less likely that villages will want to organize community freezers.[56]

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[50] Other forms of protest that have been linked to the disruptive effects of modernization are social banditry and some guerrilla movements. See Eric J. Hobsbawm, *Primitive Rebels: Studies in Archaic Forms of Social Movements in the Nineteenth and Twentieth Centuries* (Manchester: Manchester University Press, 1959).

[51] Richard N. Cooper, "Economic Interdependence and Foreign Policy in the Seventies," *World Politics*, 24 (January, 1972), 172–173.

[52] Raymond Vernon, *Sovereignty at Bay: The Multinational Spread of U.S. Enterprises* (New York: Basic Books, 1971).

[53] Peter B. Evans, "National Autonomy and Economic Development: Critical Perspectives on Multinational Corporations in Underdeveloped Countries," *International Organization*, 25 (Summer, 1971), 678.

[54] For a discussion of some of the negative consequences of American firms operating in the Canadian economy, see Kari Levitt, *Silent Surrender: The Multinational Corporation in Canada* (Toronto: Macmillan, 1970). See also Fouad Ajami, "Corporate Giants: Some Global Social Costs," *International Studies Quarterly*, 16 (December, 1972), 511–529.

[55] *World Bank Operations*, p. 418.

The presence of foreign-owned firms whose existence and expansion depend upon fostering Western-style consumption patterns can seriously limit the ability of a government to shape development strategies with a maximum social impact. The pattern of saving and the pattern of consumption, to name only two aspects of economic behavior, can be significantly distorted by the advertising and blandishments of foreign manufacturers and their local emulators.[57] Yet most observers predict a dramatic rise in the numbers and size of multinational firms within the next several decades. The majority of contemporary multinational enterprises focus their productive and marketing activities in the industrialized countries. But in the future they can be expected to seek many new ventures in the Third World. The evidence about their impact on underdeveloped societies is by no means entirely negative, but the traditional view that their activities bring substantial benefits, with no attending costs to host countries, has come under increasing attack.[58] Given the increasing awareness of the negative consequences of their activities, it would not be rash to predict that some governments will adopt more stringent and, in some cases, exclusive policies against Western economic interests.

The growth of tourism by Westerners in underdeveloped countries has had a number of consequences not originally anticipated by those who see travel and personal interaction as important sources of building "international understanding." While American- and European-built and -owned tourist facilities provide some local employment, taxes, and beneficial effects to the host country's balance of payments,[59] they also tend to create ghettos for the rich, places of amusement which display ostentatious wealth, and which alienate prime land. The behavior of many tourists is no less conducive to bad impressions and to social tensions: at its worst, it can be vulgar and racist; at its best it tends toward the paternalistic. According to L. E. Braithwaite[60] the situation in the Caribbean is already dangerous: the peculiar characteristics of North American tourists simply breed resentment and antagonisms. Pollution, the rapid growth of prostitution and many other menial "service" trades to comfort the northerners, and alarming increases in delinquency involving tourists as victims are all byproducts of this developing industry. While one would not want to argue in all cases that the disadvantages outweigh the costs, this aspect of "interdependence" may also come to face limitations and restrictions imposed by some governments in the underdeveloped countries.

Finally, in a few countries, receptivity to bilateral aid programs is diminishing. The main objective of the American aid program in Brazil has been to promote a good "climate" for American investment. One byproduct of this objective is that today the more dynamic American firms dominate most of the modern parts of Brazilian industry. Both Brazilian intellectuals and government officials are becoming increasingly suspicious of the consequences of American aid.[61] No government is likely to look with favor upon a situation where important sectors of their country's economy become dominated by foreign interests. To the extent that foreign aid programs are used as a vehicle to promote private investment, we could expect some diminution of enthusiasm to participate in them. Moreover, there is some evidence that economic aid does not promote economic growth and, indeed, in the case of Latin America, that aid may retard growth.[62] Trade and private investment rates between the United States and Latin America, according to another study, also correlate negatively with economic development: the greater the economic dependency of South American countries, the lower their growth rates.[63]

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[56] Ivan Illich, "Outwitting the 'Developed' Countries," *New York Review of Books*, 13 (November 6, 1969), 20. For a more general discussion of the negative cultural consequences of multinational firms operating in underdeveloped countries, see David Osterberg and Fouad Ajami, "The Multinational Corporation: Expanding the Frontiers of World Politics," *Journal of Conflict Resolution*, 25 (December, 1971), especially 461–468. Concerned with the problem of distorted consumption, the government of Tanzania has prohibited the importation of automobiles for private use. See Woodhouse, "Re-visioning the Future of the Third World," p. 28. In February 1973, Tanzania began prohibitions against effectuation of certain Western dress habits, such as long hair for males and short skirts for females.

[57] For a discussion of the negative cultural impact of American television advertising in Latin America, see Jack N. Barkenbus, "Communication and Cultural Change: Experience in Latin America," paper presented to the meeting of the International Studies Association, New York, March 1973.

[58] Disenchantment among some non-Marxist economists with many of the liberal solutions to underdevelopment is discussed briefly in Cohen, *The Question of Imperialism*, pp. 169, 218.

[59] The net foreign exchange earnings brought by tourism vary greatly from country to country. But in many of the underdeveloped countries, the foreign-owned tourist industry must bring in management, building supplies, and many operating items, including food and beverage. In these countries net foreign exchange earnings are usually from 45 per cent to 60 per cent of gross receipts.

[60] "Race Relations and Industrialization in the Caribbean," in *Industrialization and Race Relations*, ed. Hunter, pp. 39–45.

[61] Samuel P. Huntington, "Foreign Aid for What and for Whom?" *Foreign Policy*, No. 1 (Winter, 1970–71), 186–188.

[62] Keith Griffin, *Underdevelopment in Spanish America* (London: Allen and Unwin, 1969), p. 124.

Bits and pieces of evidence such as those listed above do not constitute in any sense verification of a hypothesis. They do suggest that many of the traditional liberal assumptions about the development of a world order through increased interdependence are being questioned.

The responses of the developed countries to autarchic or isolationist policies are difficult to predict, but it is tempting to assume that in the short run they will be more hostile than indifferent or supportive. Governments of developed states have usually retaliated in instances where important economic interests were expelled, nationalized, or even restricted in their activities. Moreover, the economic charitableness typical of the great powers in the postwar years may be waning as concentration on economic problems and safeguarding economic interests replaces fixation on the Cold War and issues of national security conceived in military terms. Underdeveloped states which undertake to restrict investment opportunities or the marketing activities of multinational firms thus may invoke not only the attention of Western governments and populations, but hostile responses as well. (Of course when a relatively noninvolved state such as Burma acts individually, few international repercussions will ensue.) But if the scenario described in the paper involves a reasonably large number of states within a fairly short period of time, the consequences to the interests of industrialized countries might be much more severe.

An alternative response, perhaps more likely in the long run, is indifference. The governments of many industrialized states, deeply concerned about social trends in their own countries, may understand that all of the orthodox solutions to the problem of underdevelopment have to be reexamined. In the meantime, aid policies would be reformulated by both donors and hosts to concentrate more on social development, agriculture, and population control and less on heavy industrialization and activities which bring advantages primarily to the modernized urban middle classes.

Whatever the hypothetical responses, there is little in the present literature on international conflict to suggest probabilities. We know a great deal about decision-making behavior in crises, the relationship between national attributes and conflict behavior, and the actions and reactions of governments in an arms race. Our conflict studies are based for the most part on the interaction of parties who are locked into a system involving high densities of verbal and symbolic communication. We know a great deal less about isolationist impulses, about situations where one state is attempting to break away from dependence upon others, or in other ways to decrease the degree of integration with the outside world.

Our visions of future international systems generally assume greater transaction flows, an increase in nongovernmental associations, and a burgeoning "international culture."[64] But before we can accept these models as possible norms for the future, we should ask many empirical and normative questions: How are the benefits of increased transactions being distributed? What are the sources of values in the "international culture?" Is the international system tending toward cultural and material homogeneity or toward acceptance and promotion of pluralism? Is homogeneity of life-styles, economics, and views of man and human activity something to be promoted or deplored? Is increased international economic integration or interdependence consistent with the preservation of the most meaningful parts of indigenous cultures? Are the networks of communication and economic activity that surround the globe capable of promoting and sustaining two-way movement, or have the networks developed in such a manner as to assure the predominance of Western and socialist states over the Third World? These and many other questions have not yet been raised by systems theorists of international relations or by those who are concerned with development problems. If the scenario in this paper has validity, then questions such as these should begin to command our attention.

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[63] Lawrence R. Alschuler, "Satellization and Stagnation in Latin America," paper presented at the European Consortium for Political Research, Mannheim, West Germany, April 1973.

[64] For example, John Burton, *Systems, States, Diplomacy, and Rules* (London: Cambridge University Press, 1968); George Modelski, *Principles of World Politics* (New York: The Free Press, 1972).

# The Primary Goals of Political Parties: A Clarification of Positive Theory

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Positive theorists, notably Anthony Downs, William Riker, and their followers, have made a major contribution to the theory of political parties.[1] Apart from the validity of their specific conclusions, a signal virtue is their method which requires clarity and precision about the fuzziest aspects of political parties. Nothing is more obscure, for example, particularly with respect to American parties, than who is included within the party. Most writers have treated the concept of membership with considerable flexibility, expanding or contracting the concept to suit their immediate purpose.[2] But the positive theorist has been more exact. Viewing the party as a calculating instrument, he explicitly decides on who is doing the calculating. Thus, whereas most analysts of political parties at one time or another, for one purpose or another, include voters within the party, Anthony Downs never does. He consistently views parties as "teams" of office-seekers.[3]

Or take the thorny problem of party goals. Parties attract a great variety of people: opportunists, power brokers, idealists, ideologues, patronage seekers, dilettantes, each with his own goals. The thorough descriptive analysis reflects this complexity.[4] It thereby provides convenient alternative explanations of a party's past behavior. Traditional descriptive analysis arrives at an impasse, however, in trying to predict the direction of party activity. The positive theorist, on the other hand, acknowledges that a multiplicity of goals provides too many variables. Typically he isolates one goal (for Riker and Downs it is "winning") and subsumes or ignores the alternatives. Such simplification makes feasible speculation about a party's future behavior. Because his goals are held only by individuals, the positive theorist also avoids the sin of reification, common in party literature, or the imputation of goals to a collectivity such as a political party.

In effect the positive theorist allows us to hold constant a complex reality, particularly those difficult aspects whose definitions depend on the observer. By being explicit and restrictive about such problems as party membership and party goals, positive theorists allow us the hope that we can accumulate knowledge about these problems, knowledge which is lacking despite the prodigious efforts of many competent scholars. It is not accidental that those aspects of party behavior about which we have been able to accumulate knowledge are precisely those which do not depend upon each observer's definition. In the last thirty years we have made considerable progress in our understanding of voting behavior. This has been possible because, despite differences in theories and techniques, no one argues about what a vote or a voter is. There now exists, therefore, an accumulation of data on voting behavior which can reliably serve theoretical speculation. But where is a similar body of data on the boundaries of the Republican and Democratic parties or on their goals? Perceptive writers such as Key, Chambers, Epstein, Sorauf, and Burnham[5] have discussed such matters in important ways, but the cumulative results of their writings on parties are nothing like the advances made in our knowledge of voting. Positive theorists, not only because they must be explicit, but also because they consciously seek to build upon each other's work, have opened the way for the accumulation of data and conclusions about the vexing problems central to the study of political parties.

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[1] I use the term "positive theory" following the example of William H. Riker and Peter C. Ordeshook, who have synthesized much of this literature in their book, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973).

[2] A thorough review of the varying scholarly meanings for the term "party" is presented in Austin Ranney's "The Concept of 'Party'," in *Political Research and Political Theory*, ed. Oliver Garceau (Cambridge: Harvard University Press, 1968), pp. 143–162. The solution of V. O. Key, Jr., who probably ranks as the most influential single writer on American parties, to the problem of membership was to speak of the "party-in-the electorate," "party-in-government," and "party of organized workers"; see *Politics, Parties and Pressure Groups*, 5th ed. (New York: Crowell, 1964), pp. 163–165. An alternative solution grades members or participants according to degree of activism from leaders to occasional supporters. See Austin Ranney and Willmoore Kendall, *Democracy and the American Party System* (New York: Harcourt, Brace and Company, 1956), pp. 199–212.

[3] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), pp. 25–26.

[4] Many studies attest to the complexity of attitudes of participants in parties. One of the most thorough is Samuel Eldersveld's *Political Parties, A Behavioral Analysis* (Chicago: Rand McNally, 1964).

[5] Key, *Politics, Parties and Pressure Groups*, chap. 8, pp. 199–227; William N. Chambers, *Political Parties in a New Nation* (New York: Oxford University Press, 1963), pp. 45–51; Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton and Company, 1970), pp. 91–134; Frank J. Sorauf, *Party Politics in America*, 2nd ed. (Boston: Little, Brown and Company, 1972), pp. 7–27; Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger Publishers, 1967), pp. 3–18.

My purpose in this paper is not to praise positive theorists. It is, rather, to demonstrate that their works, which deal almost exclusively with strategies, also can provide us with insights into the broader problem of party organization. In order to do this, however, we must first recognize that in their efforts to build a cumulative theory positive theorists have obscured vital differences in the types of parties they were examining. I shall start with the basic works of Riker and Downs, demonstrating that although the former sought to build upon the latter's theory, both were talking about different goals and different boundaries and therefore different types of party organization.[6] I shall also argue that in the discrepancies between them we can find a satisfactory explanation of significant tensions inherent in all democratic parties.

The broad outline of Downs's and Riker's theories is quite familiar. Downs's book, published in 1957, offers a model of the democratic system in which the key element is competition between rational parties for the votes of equally rational citizens. He is clear in defining a political party as " . . . a team of men seeking to control the governing apparatus by gaining office in a duly constituted election."[7] All members of the team share the same goal which " . . . is to reap the rewards of holding office *per se*."[8] Riker's theory, appearing in 1962, is less explicitly a theory of parties than it is of coalitions, of which he considers parties to be an important subspecies. The nub of his theory is the "size principle," or the proposition that under certain conditions "participants create coalitions just as large as they believe will ensure winning and no larger."[9] Proofs of the theorems of each theorist rest upon specific limiting conditions, such as perfect information, not likely to be found in the real world. Nevertheless, both Downs and Riker imply that their theories should be predictive. Thus Riker analyzes specific instances in American political history when one party came close to monopolizing the vote, as in 1820 or 1872, and argues that the party then acted in accordance with the size principle to approximate the minimal winning coalition. At no point does Riker state explicitly what he means by a political party or even what the goal of a party is, other than "winning." But since he discusses Downs's party and applies his own principles to it, we assume that he believes they are talking about the same thing.

There is, however, an obvious point at issue between them: the size of the electoral margin which the party seeks. Downs states it as axiomatic that a party seeks to maximize its vote. Riker's size principle, on the other hand, leads him to conclude that a party seeks to win by the smallest margin possible, or to achieve the "minimal winning coalition." Riker argues that had Downs adopted the minimal winning strategy he would have avoided his crisis in rationality. The rationality crisis in Downs's theory emerges because parties, in their effort to gather as many votes as possible, become ambiguous in their policies, thus making it impossible for voters to behave rationally. What is rational for the party leads to irrationality in the voter. Riker points out that if parties properly follow the minimal winning strategy they no longer need to be ambiguous. Thus the rationality crisis is resolved.[10]

For some time Riker's criticism of Downs appeared reasonable to me, and, so far I can discern, to others for whom this was a matter of interest. After all, the proposition that parties try to win by as narrow a margin as possible had been made very effectively by E. E. Schattschneider twenty years earlier in his enormously influential book, *Party Government*.[11] To those of us who prefer to see politics as the interaction of rational beings it was a relief to see any rationality crisis resolved.[12] Yet in doing so we overlooked the possibility that Riker and Downs were not really talking about the same thing; that what each meant by "party" was different and that from these differences there followed quite distinct yet equally rational strategies.

My own realization that Riker's size principle was not a general theoretical principle applicable to all parties came from a practical experience which is worth recounting briefly. In the fall of 1971 I served as campaign manager for a candidate running for city council. The candidate was an incumbent seeking re-election in a difficult situation. The extension of the vote to eighteen-year-olds had more than doubled the electorate in a university town. A well-organized, radical anti-incumbent slate was fully prepared to use the untested youth vote to oust my sixty-year old incumbent, despite his liberal credentials. As a liberal councilman, however, he had antagonized many of the older town voters who were supporting a conservative slate. As the middleman between two opposing forces, our candidate was able to squeak through by the narrow margin of forty votes out of some 14,000 cast.

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[6] William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962) and Anthony Downs, *Economic Theory of Democracy*.

[7] Downs, p. 25.

[8] *Ibid.*, p. 28.

[9] Riker, pp. 32–33.

[10] *Ibid.*, pp. 98–101.

[11] E. E. Schattschneider clearly anticipated the reasoning of positive theorists, including Downs and Riker. Anticipating the concept of the minimal winning coalition, he wrote that a party would not want to win 100 per cent of the vote because "it is unnecessary and wasteful. Fifty-one per cent of the vote will give any party all there is of the power to govern. . . . From the point of view of the interests participating in the political venture, it is more profitable to share a victory with a narrow majority than it is to partake of the spoils of victory with a larger number, for the smaller the number of participants the greater will be the share of each . . . the perfect party victory is to be won by accumulating a relatively narrow majority, the mark of the skillful conduct of politics." *Party Government* (New York: Holt, Rinehart and Winston, 1942), pp. 95–96.

[12] Kenneth A. Shepsle, "The Strategy of Ambiguity: Uncertainty and Electoral Competition," *The American Political Science Review*, 66 (June, 1972), 555–568, presents a different resolution to Downs's rationality crisis.

In the postelection euphoria we naturally expounded on the brilliance of our strategy. Any more effort, one more sign, one more mailing, or one more promise would have been wasted; any less effort would have spelled defeat. Although the brilliance of the strategy was evident only in retrospect, the fact remains that had we known what we were doing (i.e., enjoyed Riker's condition of perfect information), we would have followed precisely the same strategy; that is, if we accept the logic of the minimal winning coalition.

Yet a strange thing began to occur which our reading of Riker had not led us to expect. When talking to campaign workers and voters it soon became apparent that every one of those voters was one of the last forty and that each worker had brought in the last forty votes. Each appeared to feel, therefore, that he had a special claim. What had happened, clearly enough, was that by achieving a minimum winning coalition we had maximized the number of marginal voters. And marginal voters, precisely because their votes are the ones needed to win, have a greater claim on the officeholder. Thus the minimal winning coalition, rather than reducing the number of payoffs which the officeholder had to make, actually appeared to heighten the expectations of payoffs. Perhaps, after all, Downs really was right and we should have maximized the vote.

Upon closer examination, it seemed to me that both Riker and Downs were right, depending on whether one considered the primary interests of the candidate or of the voter. The strategy of the minimum winning coalition certainly made sense for the voters. By reducing their numbers to the minimum needed to win, they could maximize their claims on the successful candidate. But for the same reason, the candidate was well advised to maximize the number of his voters, if only to weaken the claims that any one voter could make upon him. There was then a conflict of strategies.[13] From that conflict Riker emerged as the theorist of the voters, Downs the theorist of the candidate.[14]

To go one step further, if Riker and Downs have provided us with two equally rational but distinct political strategies, does their work not also imply two distinct answers to the problems of party goals and party boundaries? For Downs a party consists only of office-seekers, and its goals are confined to their goals, namely the winning of office. For Riker a party must encompass the voters as well, for they are part of his coalition. Confusion arises because Riker wants his party to be the same as Downs's. He fails to recognize that by admitting voters and their goals to the party, he must change almost all of Downs's assumptions. In particular he must reverse the critical proposition that " . . . parties formulate policies in order to win elections, rather than win elections in order to formulate policies."[15] Voters find no advantage in winning elections per se; office is a payoff limited strictly to candidates. The party which encompasses the voter must derive its interest in winning elections from the hope that it will thereby be able to implement policies or other nonofficeholding benefits for its members.

It would be unfair not to note here that Riker is quite aware of the distinction between the payoff to leaders and the payoff to followers in electoral coalitions. But in trying to resolve the conflict he resorts to psychoanalytic explanations of the motives of leaders, a most uncharacteristic lapse from his rationalist assumptions. For example, he relates Woodrow Wilson's extraordinary efforts to gain office to his need to overcome feelings of inadequacy instilled by his father. In so doing, Riker arrives at a rationality crisis of his own, for he concludes that " . . . leaders may pay out more to win than a victory is objectively worth."[16] Greater clarity will be achieved, I believe, if we recognize that we are dealing with two potentially conflicting goals each of which implies not only its own strategy but also its own type of party organization.

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[13] Even though a minimizing strategy makes sense for voters, they are hardly capable of carrying one out. To pursue such a collective strategy requires organization, a point to which I return below. Candidates, on the other hand, can seek a maximizing strategy, as has been demonstrated by Marjorie R. Hershey, "Incumbency and the Minimum Winning Coalition," *American Journal of Political Science,* 17 (August, 1973), 631–637.

[14] The economist, George J. Stigler, in "Economic Competition and Political Competition," *Public Choice,* 13 (Fall, 1972), 91–106, has sought to resolve the conflict between party objectives found in Downs and Riker. While his argument is interesting, it rests upon the complete elimination of winning office as a party goal, a most apolitical assumption.

[15] Downs, *Economic Theory of Democracy,* p. 28.

[16] Riker, *Theory of Political Coalitions,* p. 209. The discussion of special payments to leaders is in pp. 203–210.

### The Two Primary Goals

There are two kinds of people who will seek to participate in political parties to achieve two distinct goals: (a) *office-seekers* whose goal is *office* and (b) *benefit-seekers* who want *benefits* derivable either from the control of office or in some way dependent upon the existence of the party. Downs's theory is about the former, Riker's about the latter. The two goals must not be confused because they lead to distinctive strategies and conflicting ways of organizing parties. Riker has obscured the differences by subsuming both goals under the term "winning." Winning, however, is not a primary goal; it is a means of gaining something else. True, there may be some people for whom politics is like amateur competitive sports; for them there is adequate satisfaction in playing the game in the hope of winning. But I would suspect that for most participants winning in politics means gaining either office or the benefits derivable from the control of office.

It is the inherent value of *office* which is most obscured by Riker's argument. Yet its value is certainly a commonplace assumption. The widely held view that most if not all politicians are "opportunists" rests on the notion that it is the desire for office and its powers that guide officeholders rather than any concern for the content of public policy. Most political systems go to great lengths to give office independent value. Terms of deference such as "your honor," the lifetime use of titles such as "Mr. President" or "Governor," graded seating arrangements at state functions, and the privilege of state funerals all remove those who hold political office from the ordinary and give office holding a value well beyond the material benefits which may accrue to office holders. We watch wealthy men spend vast sums of money in the quest for offices which could not possibly provide them with a commensurate material return. History does not allow us to ignore the power of ambition for office as an incentive in its own right. Ambition is also a distinctly private or individual incentive which such positive theorists as Mancur Olson have taught us is essential to the development and maintenance of any organization as large as a political party.[17] True, only a few people can ever reasonably hope to hold office, but this does not reduce the central importance of the goal of office for the development of political parties.

By *benefits*, on the other hand, we mean all the nonoffice goals which people bring to politics. Thus benefit-seekers include anyone who hopes to affect government activity. Many benefits sought are private in character: jobs, contracts, or individual favors. Others are collective, such as the national defense or control of atmospheric pollution. Hence we include under benefits policies which provide the most selfish and private of advantages and those which derive from a completely selfless concern for the common welfare. We bring both types of concerns together under the general rubric of benefits because we wish to demonstrate that the strategies of all benefit-seekers have more in common with each other than they do with the strategies of office-seekers.

We know, of course, that a political party is only one means by which people seek political benefits. Commonly we call organizations which seek benefits without seeking elective office interest groups. Such groups may employ a wide range of tactics. When, however, benefit-seekers form a party or enter an existing one, they commit themselves to some use of the tactic of electoral politics. Since winning office or being able to affect someone else's chances of winning is central to electoral politics, benefit-seekers must decide that such a tactic is useful before they enter party politics. American labor unions, for example, debated this issue for many years before deciding during the 1930s that working for the election of Democratic candidates was a suitable strategy.[18]

We should note also that for many benefit-seekers, even those within political parties, winning office is not essential for the achievement of all of their objectives. Merely by entering competitive politics benefit-seekers force the competition to pay attention to their demands. If, in turn, by acceding to these demands the opposition wins the victory, the benefit-seekers, while having lost the office, have gained the concessions at least.

Nevertheless, winning elections, while not essential to benefit-seekers, is a better strategy than not winning. In 1968 George Wallace's American Independent party failed to win the election, but the size of its vote and the threat of its re-emergence in 1972 forced the two major parties to make policy concessions to its supporters. These concessions, however, were only partial. Without doubt, Wallace and his supporters would have gained more of their desired benefits had they won. As for office-seekers, winning elections is essential, the *sine qua non*, which forces them to form or enter political parties. The fact that winning is the mandatory strategy for one goal and only the optimal strategy for the other is an important distinction.[19] But here I wish to emphasize that winning elections is the best strategy for both primary goals.

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[17] Mancur Olson, *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965).

[18] On the debate within the labor movement as well as the impact of direct involvement on the unions' own political flexibility, see J. David Greenstone, *Labor in American Politics* (New York: Alfred A. Knopf, 1969).

If winning is the best strategy for both office-seeker and benefit-seeker, is it useful, after all, to distinguish between the two primary goals? Surely benefit-seekers and office-seekers need each other. Office-seekers must be concerned with benefits; otherwise no one would have any reason to work for them. At the same time, as I have pointed out, benefit-seekers realize their maximum gains either by winning office or by supporting those who do. True, one's ends may well be the other's means. But is it possible to disentangle means from ends in the real political world? On the basis of the experience which I recounted earlier I would contend that it is possible because there are distinct types of behavior. In other words, while both office-seekers and benefit-seekers seek to win elections, they do so in different ways. It is in trying to distinguish among such strategies that the theories of Riker and Downs can give us guidance.

First, however, it is important to include a refinement which has already been added to Riker's and Downs's work. As I pointed out earlier, the distinction between Riker and Downs rests on the size of the victory which each writer says a party should seek. But the size of a party's victory is not as simple a matter as it would appear from the early writings of the positive theorists. As Hinich and Ordeshook have pointed out, Downs and others failed to distinguish clearly between maximizing one's vote and maximizing one's plurality.[20] While at first glance, these may appear to be the same, they in fact can differ considerably if voters abstain. The ability of voters to decide not to participate makes it possible for a party to try to maximize its plurality, or the distance between it and its nearest competitor, while at the same time minimizing its total vote. Or a party might opt for maximizing its vote while minimizing its plurality. In any event, only if all citizens vote can we say that vote maximizing and plurality maximizing are the same thing.

Recent political history provides us with ready evidence that the maximization of a party's plurality and the maximization of its vote do not necessarily go together. In 1960 Richard Nixon barely lost to John Kennedy in the popular vote; Kennedy's plurality margin was only 0.2 per cent. In 1960 Nixon's percentage of the *potential* vote was 31.6 per cent. In 1972 Nixon achieved a landslide plurality over McGovern of 23.2 per cent. Yet because the turnout rate was well below that of 1960, Nixon's percentage of the potential vote had risen only a little over two points to 33.7 per cent.[21]

If we add to the maximizing objectives of parties the refinement of the size principle, we can see four distinct strategies which political parties can follow, depending on how they combine the two components of victory. Two factors make up the size of any victory: (a) the size of the vote received, and (b) the size of the plurality. Logically they are independent of each other. A party can win all of the votes cast and gain a plurality of 100 per cent while winning hardly any of the potential vote, a situation characteristic of many an uncontested election. Any combination of the two variables is at least possible. I have chosen, however, to concentrate upon the extremes in which the parties seek either to maximize or minimize the two variables.

### The Four Strategies

(1) The first strategy requires the party to *minimize its plurality, while minimizing its vote.* This is the benefit-seeker's best strategy. It is identical with Riker's minimum winning coalition, for it seeks to reduce the size of one's support to the bare minimum. The benefit-seeker wishes to minimize the vote or seeks a low turnout election in order to minimize the number of claimants to the

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[19] The fact that office-seekers must win while benefit-seekers in parties find winning the election only the best alternative is one reason that the former tend to dominate parties. Lacking an alternative, the office-seeker must either keep pressing his strategy, give up his goal, or move on to another party. The benefit-seeker, because his objectives are likely to be satisfied in some measure, is under less pressure to insist upon his strategy. He may even be able to withdraw from direct partisan activity and remain a benefit-seeker, something an office-seeker cannot do with respect to his goal.

In addition, the office-seeker and everyone else know when his goals have been achieved. The achievement of benefits, however, is not always so clear. The fact that one goal—office—is simple, observable, and easily measured in its achievement, while the other—benefits—is usually ambiguous and always subject to argument over its achievement reinforces the tendency for the office-seeking goal to dominate political parties, as well as the attitudes of candidates themselves.

[20] Melvin J. Hinich and Peter C. Ordeshook, "Plurality Maximization vs. Vote Maximization. A Spatial Analysis with Variable Participation," *The American Political Science Review*, 64 (September, 1970), 772–791.

[21] One positive theorist has addressed the problem of relating turnout to plurality. See Gerald H. Kramer, "A Decision-Theoretic Analysis of a Problem in Political Campaigning," in *Mathematical Applications in Political Science II*, ed. Joseph L. Bernd (Dallas: Arnold Foundation, Southern Methodist University Press, 1966), pp. 137–160.

spoils. At the same time, the benefit-seeker wants to minimize the plurality of the victory for the reason I pointed out earlier: the narrower the victory, the greater the claim of each supporter upon the successful candidate. Reducing the number of voters reduces the number of people among whom the benefits must be shared; reducing the plurality strengthens the hold of each supporter upon the officeholder. Again we must emphasize that the argument holds, regardless of the type of benefit being sought. Patronage seekers hanging around the party clubhouse are chary of newcomers. Ideologues watch their candidate closely for signs of a broadening appeal, or in their words, "selling out." Both recognize the value of minimizing support in order to maximize their benefits.[22]

(2) The second strategy consists of *maximizing both the number of votes and the plurality.* It is the most advantageous method of winning for the office-seeker. Large victories enhance the inherent values of office. The leader of a coalition maximizes his independence from any individual or group. If the winner using the minimizing strategy emerges as a delegate, the winner who maximizes his victory comes forth as the trustee, the true leader. Yet it is more than the value of a particular victory which leads an office-seeker to use a maximizing strategy. It also improves his chances at continuing or advancing in office. The freedom he gains from past supporters permits him to maneuver for advantage in future elections. Since a political career is based on the continuous search for office, office-seekers must be future oriented, concerned with being right in future elections and therefore relatively free of the burdens imposed by elections in the past. Thus a politician who has successfully maximized his victory can more readily shift his stance as the flow of political issues requires it.

Maximum victories also serve office-seekers as a kind of political currency. The political community—including party officials, newspaper columnists, and financial contributors as well as other office-seekers—is visibly impressed by large victories. Politicians who get themselves talked about as potential presidents, governors, or senators are usually those who have been able to demonstrate broad popular support. Political parties which bring together the office-holding ambitions of many politicians are thus drawn even more than individual candidates to the maximizing strategy.[23]

(3) The remaining strategies are mixed strategies or compromises. One consists of *maximizing one's plurality while minimizing the vote.* If he must compromise, this is probably the compromise the office-seeker would prefer. Davis, Hinich, and Ordeshook present plurality maximization as the best strategy for candidates.[24] My argument is that they are also served by maximizing the vote. But if an office-seeker cannot have both dimensions of victory it is likely that he would sacrifice the size of the vote. My guess, for example, is that John Kennedy would have preferred to win as Nixon did in 1972 rather than as he himself did in 1960, that is, by a large plurality in a low-turnout election rather than by a narrow plurality in a high-turnout election. Narrow pluralities not only reduce the maneuverability of the winner, they pose a threat to winning the office itself. Of course under conditions of perfect information the threat would not exist; but given the normal conditions of uncertainty we can expect the reasonable office-seeker to worry first about his plurality. Furthermore, by not seeking to expand the vote, the plurality-maximizing office-seeker can assuage his benefit-seeking supporters who, as we have pointed out, are not interested in seeing their claims diluted any more than necessary.

The Nixon campaign of 1972 is an example of what appears to have been a plurality-maximizing, vote-minimizing strategy. Nixon's large plurality was produced by sizable shifts of traditionally Democratic voters, southern whites, blue-collar workers, and ethnic minorities, all of whom were disaffected by the direction which the Democratic party had taken since 1964. Nixon, however, made few appeals to new voters, particularly to those who characteristically turn out in low numbers: blacks, poor whites, and the young. Indeed, Nixon himself did hardly any campaigning at all. In the southern states, where he was to register enormous pluralities, he followed for the most part a strategy of noninvolvement in lesser races. A brisk campaign would undoubtedly have relieved much of the tedium many observers noted among the electorate and would surely have produced a larger turnout. In retrospect, at least, it was a strategy well calculated to maximize Nixon's plurality, while keeping the overall vote low.

It is important at this point to recognize that the two elements, plurality and size of vote, are independent of each other only in a technical sense. A candidate such as Richard Nixon might reasonably conclude that his chances for a sizable plurality rested on a low turnout of voters. John Kennedy in 1960, on the other hand, explicitly waged a get-out-the-vote campaign because he reasoned that this would enlarge his plurality. Depending on the reasons for abstention, we know that variations in turnout do not affect each party equally. Thus depending upon the candidate and the issue, each party must decide what combination of strategies it should follow. This in no way vitiates our central point, however, that the preferred strategy of the benefit-seeker is to minimize both turnout and plurality, that of the office-seeker to maximize both.

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[22] The proposition that benefit-seekers want a minimal victory rests on the assumption that control of the office can produce the benefit. The American system, combining separation of powers with bicameralism and federalism, means that control of one element does not necessarily produce the desired benefits. The need, therefore, to control more offices may lead the rational benefit-seeker to adopt a more than minimum winning strategy.

[23] On political parties as coalitions of ambitious office-seekers, see Joseph A. Schlesinger, *Ambition and Politics* (Chicago: Rand McNally, 1966).

[24] Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *The American Political Science Review*, 64 (June, 1970), 438.

(4) The second compromise strategy, *minimizing one's plurality while maximizing the vote*, fits the needs of benefit-seekers more than of office-seekers. The latter would find much of the value of the large vote wiped out by the small plurality. A large vote alone would mean that the number of marginal voters had been maximized, placing an office-holder in the weakest position. The type of benefit-seekers, however, who might find this to be a desirable compromise are those who have a commitment to maximum voter participation, groups such as the League of Women Voters, many political scientists, and advocates of greater participatory democracy.

The two primary goals of political organization, as well as the two types of people we have described as participants in party politics, are, of course, abstractions. For that reason the strategies of compromise may indeed be the more realistic. Our ambitious office-seeker is not likely to be concerned solely with office. Inevitably he is a citizen concerned with the policies of government. To that degree, he too is a benefit-seeker. As office-seeker he wants to free himself through maximizing his victories. Only a sizable victory frees him to pursue any policy and join any coalition in his quest for advancement in office. As a benefit-seeker, however, he may well wish to reduce his maneuverability; thus he is drawn to a minimizing strategy. Like the southern congressman who could do pretty much as he wished as long as he voted the southern position on racial legislation, the benefit-seeker in office wants to be held fast by his supporters to those policies which will produce the benefits he wants too. The claims of his supporters provide him with the best defense against the vote-trading demands of other legislators.[25] And yet, the office-seeker within him wants to be able to make those trades when they can help further his career.

In the real political world, therefore, both the individual and the party are inevitably subject to the pressures of both goals. The tensions within individuals and parties between the goals of office and benefits, as well as the confusion over whether offices and benefits are ends or means, make it nearly impossible to determine which goal is dominant through observation. Yet in practice we do it all the time, because we discern differences in strategies. On what basis, for example, do most observers conclude that the major American parties are predominantly concerned with winning office? I submit that this conclusion rests far less on any overt confession in party platforms or in the public utterances of candidates, than in our observation that the parties follow vote- and plurality-maximizing strategies, which make sense only if the quest for office is uppermost. We conclude, on the other hand, that some candidates are primarily ideological or oriented toward benefits because they take actions which appear exclusionary. That is, our test of ideological or issue orientation is a candidate's willingness to adopt what we see as a minimizing strategy. Thus we watched Eugene McCarthy cling to his "smarter" voters, Barry Goldwater drive away all but his preferred "extremists," and George McGovern's supporters reject as unacceptable practitioners of the "new politics" large segments of the traditional Democratic coalition. While we can never know with certainty the motives of politicians, our assumptions about their motives rest upon the strategies they employ.

The distinction among different strategies of electoral competition has certainly not been ignored by positive theorists. We are indebted to Hinich and Ordeshook[26] for their clarification of the consequences of vote- and plurality-maximization. Hinich and Ordeshook treat these strategies, however, as if these were the primary goals of candidates and parties rather than their means. The authors do so because they are concerned with the fact of winning, without asking what it is that the candidate wins. As I have sought to demonstrate, these are partial strategies which may be combined in various ways with vote- and plurality-minimizing as well. A complete theory of electoral competition has to take these alternatives into account also.[27]

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[25] The benefit-seeking officeholder's problem is not unlike that analyzed by Thomas C. Schelling in which a bargainer increases his strength by proving his inability to change his position. *The Strategy of Conflict* (New York: Oxford University Press, 1962), pp. 22–28.

[26] Hinich and Ordeshook, pp. 772–791.

[27] Positive theorists have examined possible goals for parties and candidates other than plurality and vote maximization. Riker and Ordeshook, "Plurality Maximization vs. Vote Maximization," p. 336, mention briefly such goals as maximizing the proportion of the vote, the probability of winning, and the probability of securing a fixed percentage of the vote. None of these, however, is identical with the minimal winning coalition.

The failure of the positive theorists to probe further and to define the goals of parties as office and benefits accounts for their inability to integrate the principle of the minimum winning coalition into their theory of electoral competition. There can be no doubt that Riker means his size principle to be a statement about the objectives of candidates and parties. In the work, however, in which he and Ordeshook seek to synthesize positive political theory, the size principle and the discussion of electoral competition are handled as though they were entirely unrelated.[28] If parties do seek to attain a minimum winning coalition, as they argue in chapter 7, then one might expect the coalition to play a major part in their discussion of candidates' objectives which appears in chapter 11. Instead it is relegated to a footnote which asserts that under certain conditions the minimum winning coalition leads to results identical to those of plurality maximization.[29] While it is true that the results can be identical, Riker and Ordeshook fail to point out that a candidate who is trying to maximize his plurality can hardly be trying to minimize his coalition. In order to incorporate the principle of the minimum coalition into competitive electoral theory we must recognize that parties and candidates can follow both minimizing and maximizing strategies.[30]

It is one thing, of course, to develop a theory of strategies and another to perceive differences in party behavior in the face of competition. In a two-party system where both parties accept winning as the optimal mean of achieving their goals, under conditions of uncertainty about what will influence the outcome, it probably makes little difference whether the goal is office or benefits. Competition forces both office- and benefit-seekers to woo a similar electorate; uncertainty forces them to broaden their appeals. As a result anyone's conclusions about "real" goals is subject to dispute. I wish only to point out that conclusions about goals are drawn not from direct knowledge about a candidate's motives, but rather from what we perceive to be his strategy.

## The Implication of the Two Primary Goals for Party Organization

The importance of the distinction between the goals of office and benefits rests, therefore, not in its usefulness in predicting how parties will behave but in determining how parties are organized. It is precisely because positive theorists have failed to distinguish between the two goals that their impact upon the study of political parties has been limited. Just as Riker believed that his party was the same as Downs's, students of parties have generally concluded that positive, or rational calculating theories apply only to office-seeking parties. For example, William Wright has classified parties and party theorists along a continuum ranging, in his words, from "rational-efficient" to "party democracy."[31] At one end of the continuum he places the major American parties and theorists such as Downs; at the other end of the continuum are the European Socialist parties and such theorists as Duverger. Clearly Wright assumes that only office-seekers are either capable of facing or willing to face the electorate in a rational, calculating manner. Were he to drop that assumption, his own continuum would become more reasonable, for the distinctions between parties would be reduced to the distinction between parties organized to gain office and those organized to maximize benefits.

The assumption that only office-seekers are rational calculators is, however, a bias which runs deeply through the study of politics. For example, James Q. Wilson's influential distinction between amateurs and professionals rests on the notion that only the latter are capable of treating issues and votes in a detached manner.[32] Interested in winning office, they can compromise and bargain. The amateur, on the other hand, holds policies as pre-eminent. He feels that decisions should always be based on principle and thus is driven toward extremist positions. The difference is one of style and personality: the professional is the shrewd, rational, calculating politician, the amateur, the hot-blooded, do-or-die man of principle.

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[28] Their discussion of the size principle is found on pp. 176–201, assumptions about the objectives of candidates on pp. 333–337, in *Introduction to Positive Political Theory.*

[29] *Ibid.,* p. 335, fn. 33.

[30] Positive theorists have examined the possibility that candidates and parties are subject to internal restraints, i.e., their ability to respond to electoral competition may be affected by the need to respond to activists either in obtaining nominations or campaign resources. Riker and Ordeshook, raise the problem briefly on pp. 361–362. It is also raised in Davis, Hinich, and Ordeshook, "An Expository Development," pp. 426–448 and in James S. Coleman, "Internal Processes Governing Party Positions in Elections," *Public Choice,* 11 (Fall, 1971), 35–60. The argument of these articles is quite distinct from the one I am making since they treat activists as people who have *preferences,* while only candidates or parties have *strategies.* Thus their analysis is similar to James Q. Wilson's distinction between amateurs and professionals. My contention is that the activist is capable of strategic judgments; a primary voter can be concerned with choosing the candidate likely to win in the general election as much as with choosing one who comes closest to his preferences.

Two articles which do treat the activist as a potential strategist are Peter H. Aranson and Peter C. Ordeshook, "Spatial Strategies for Sequential Election," in *Probability Models of Collective Decision-Making,* ed. Richard G. Niemi and Herbert F. Weisberg (Columbus: Charles E. Merrill, 1972), pp. 298–331, and James S. Coleman, "The Positions of Political Parties in Elections," in Niemi and Weisberg, pp. 332–357. These do not, however, develop my point that the activist as benefit-seeker attempts to win with a strategy different from that of the office-seeker.

[31] William E. Wright, *A Comparative Study of Party Organization* (Columbus: Charles E. Merrill, 1971), pp. 17–54.

[32] James Q. Wilson, *The Amateur Democrat* (Chicago: The University of Chicago Press, 1962).

There is no need, however, to reject rationalist assumptions in interpreting the behavior of policy-oriented participants. No *a priori* reason exists for assuming them to be less capable than office-seekers of treating issues and voters in a detached, calculating manner. Indeed, Robert Putnam, in his careful study of British and Italian parliamentarians, found no barrier at all between ideological politicians and willingness to compromise.[33] We must recognize, however, that each goal implies its own strategy. Behavior that is rational for an office-seeker can be irrational for a benefit-seeker. We must, therefore, be careful not to confuse their distinct objectives under the rubric of "winning."

Since winning an election means something quite different to the office-seeker than it does to the benefit-seeker, each goal requires a distinct approach to party organization.[34] The difference in organizational effort made by the benefit-seeker and the office-seeker derives primarily from the different value which winning has for each. For office-seekers, winning provides immediate satisfaction. The state according to its rules hands over the prize. The benefit-seeker, however, must await the decisions of the officeholder. Thus the benefit-seeker needs some mechanism to assert his claim. Indeed, before he can make any claim at all, he must be able to prove his support. This is not as easy as it might appear under a voting arrangement with secret ballots. Indeed, one of the principal arguments for the secret ballot is that it makes it difficult for some types of benefit-seekers to prove their support and be paid for their vote. What good, then, is it for voters to forge a minimal winning coalition if there is no way they can prove to the successful candidate that they indeed supported him? For all he knows they might have voted for his opponent.[35] Benefit-seekers thus need proof that they did support the candidate.

There are, of course, ways in which benefit-seekers can prove their support. Giving money has been institutionalized in the United States to the extent that the office-seeker must officially report each contribution, thereby giving legal assurance to benefit-seekers that their support will be recorded. Working for a candidate is another way, and only a foolish benefit-seeker would do so anonymously. Interest groups such as labor unions are careful to identify their support and even devise ways to keep their activities distinct so that their contribution to the coalition is clear. All of these methods are available to individuals and groups willing to give up the anonymity of the secret ballot. For the ordinary voter, however, there is only one way to exert claims through a party, and that is to become an explicit party member.

Hence we arrive at the rationale for the membership party. It rests on the proposition that a party is, or should be, concerned with distributing benefits. The people for whom the party seeks benefits, therefore, must be identifiable. Voters must be turned into members. Only in this way can the minimizing strategy become effective. Thus ideological movements which become political parties must carefully define their boundaries and develop techniques such as the membership card. Similarly a party theorist such as Duverger, who conceives of party as the representative of interests, i.e., benefit-seekers, has to conclude that the mass-membership party is the wave of the future.[36] Without a membership to make claims and a clearly identified structure to enforce them, what can keep the officeholder in line?

The office-seeker, on the other hand, will naturally prefer not to be burdened with mechanisms designed to keep him in line once he has achieved the goal of office. Hence his preference for light organizational baggage, for a loose structure with vaguely defined members. He wants a large body of voters who are identified with his party and can generally be counted on for support. But there are costs to him in putting names to those voters. Obviously he needs help. Inevitably there will be people upon whom he will become dependent and who will therefore be able to make claims. But the office-seeker wants to keep such people at a minimum and easy to replace. Only in this way can he achieve the maneuverability which is one of his most valuable assets.

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[33] Robert D. Putnam, "Studying Elite Political Culture: The Case of 'Ideology'" *The American Political Science Review*, 65 (September, 1971), 669.

[34] Peter C. Ordeshook, in "Extensions to a Model of the Electoral Process and Implications for the Theory of Responsible Parties," *Midwest Journal of Political Science*, 14 (February 1970), 43–70, applies positive theory to one concept usually associated with problems of internal party organization. His discussion of the responsible party concept, however, deals entirely with the question of whether or not parties present clear and distinct electoral choices.

[35] In many political situations open voting does exist, thus making the type of organization required by voters with a secret ballot unnecessary. It is in such situations, quite naturally, therefore, that the concept of the minimal winning coalition has been applied most successfully. Thus it has been applied to presidential nominating conventions by Steven T. Brams and William H. Riker, "Models of Coalition Formation in Voting Bodies," in *Mathematical Applications in Political Science VI*, ed. James F. Herndon and Joseph L. Bernd (Charlottesville: University Press of Virginia, 1972), pp. 79–124. Legislative voting is also open so that benefit-seekers need no special structure to demonstrate their support, hence the strategy of the benefit-seeking coalition is most readily observed there. The volume edited by Sven Groennings, E. W. Kelley, and Michael Leiserson, *The Study of Coalition Behavior* (New York: Holt, Rinehart and Winston, 1970) contains a number of such studies in various countries, the common feature of which is that the coalitions are formed by individuals overtly.

[36] Maurice Duverger, *Political Parties* (New York: John Wiley and Sons, 1963).

The maneuverability which the office-seeker gains from a loosely organized party does not free him from all controls; but the controls to which he is subjected do differ from those which are exerted over the leaders of benefit-seeking organizations. Albert O. Hirschman has distinguished between voice and exit as types of organizational controls.[37] In an office-seeking organization, exit, that is the willingness of supporters, including candidates, potential candidates, and voters, to leave, controls behavior. In the benefit-seeking organization, voice or the internal expression of dissatisfaction is the means of control.

The existence of distinctive controls confirms the distinctions in organization which I have already described. Control through exit requires no elaborate structure. The departure of voters, candidates, and other contributors can become painfully obvious. Office-seekers in the Republican party after 1964 and the Democratic party after 1972 needed no referenda, polls, or conventions to determine that the party had done something wrong and must alter its course. Control through voice, on the other hand, requires an apparatus through which party members can express their sentiments and exert their claims. Continuous forums must be provided, whether through branches, clubs, or conventions. Once these forums exist, it follows that the party's officeholders must carry out in a disciplined fashion the demands which these forums make explicit.

In conclusion I have sought to expand our use of positive theory by pointing out that there are in fact two positive theories, one for benefit-seekers, the other for office-seekers. In so doing I hope I have shown how we can explain the distinction between office-oriented and issue-oriented political organizations which is so often made. But I hope also to have offered a plausible explanation for the tensions which plague all political parties. In the real political world no party consists solely of office- or benefit-seekers. Conflicts within parties arise, therefore, not simply because of ideological or policy differences, but because two goals impose conflicting strategies for winning elections and for organizing parties. Hence the distinctions in positive theory which we have sought to make help explain the never ending struggle over party reform in democracies.

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[37] Albert O. Hirschman, *Exit, Voice, and Loyalty* (Cambridge: Harvard University Press, 1970).

# Variations in Elite Perceptions of American States as Referents for Public Policy Making*

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## Introduction

Analysis of American state expenditures emerged during the 1960s as a major focus for political research. Variously called public policy analysis or output analysis, these studies attempted to relate state spending to several socioeconomic and political characteristics of the American states.[1] The results of this research generally cast doubt on the importance of political factors in the determination of state policy; social and economic indicators accounted for most of the attributable variance in the levels of public expenditures in the states. This "seemingly outrageous description of state politics"[2] led to more elaborate conceptualization and increasingly sophisticated data analysis. Even with improved methodologies, however, much of the variance in state spending remains unexplained.[3]

Missing from the state spending studies is the contribution made by elected and appointed officials to the formation of public policy in those states. One effort to provide an elite and organizational behavior context for the study of state

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[1] The first published account of the interrelationships between political party competition, socioeconomic characteristics, and state public policy outputs was Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics,* 25 (May, 1963), 265–289. Other studies which emphasize the role of economic variables in determining state spending include Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review,* 60 (March, 1966), 73–82; and Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966). Publications placing greater emphasis on the impact of political variables on state spending include Ira Sharkansky, "Economic Development, Regionalism and State Political Systems," *Midwest Journal of Political Science,* 12 (February, 1968), 41–61; Ira Sharkansky, *Spending in the American States* (Chicago: Rand McNally, 1968); and Andrew T. Cowart, "Anti-Poverty Expenditures in the American States: A Comparative Analysis," *Midwest Journal of Political Science,* 13 (May, 1969), 219–236.

For a review of these and many other related works, see John H. Fenton and Donald W. Chamberlayne, "The Literature Dealing With the Relationships Between Political Processes, Socioeconomic Conditions and Public Policies in the American States: A Bibliographical Essay," *Polity,* 1 (Spring, 1969), 388–404; and James W. Dyson and Douglas St. Angelo, "Persistent Methodological and Theoretical Problems in Analysis of Policy Outputs," paper presented at the Annual Meeting of the Southern Political Science Association, 1969.

[2] Herbert Jacob and Kenneth N. Vines, "Epilogue," in *Politics in the American States,* 2nd ed., ed. Jacob and Vines (Boston: Little, Brown, 1971), pp. 556–562. The authors point to inadequate measurement of concepts, inappropriate analysis techniques, and overly restrictive definitions of policy outputs as major sources of difficulty in many of the state expenditure studies. Supportive of this critique is Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review,* 63 (September, 1969), 858–866.

More recent attempts to expand the universe of relevant variables include Ronald E. Weber and William R. Shaffer, "Public Opinion and American State Policy-Making," *Midwest Journal of Political Science,* 16 (November, 1972), 683–699; and Sarah McCally Morehouse, "The State Political Party and the Policy-Making Process," *American Political Science Review,* 67 (March, 1973), 55–72.

[3] Ira Sharkansky and Richard I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review,* 63 (September, 1969), 867–879. See also Sharkansky and Hofferbert, "Dimensions of State Policy," in Jacob and Vines, pp. 315–353. Sharkansky and Hofferbert's research is afforded separate notation because their conclusion that different "mixes" of socioeconomic and political factors are influential in state policy making dependent upon the policy area being considered is important to our presentation. In particular, their finding that political factors are more influential with regard to the welfare and education dimension of state policy formation than with the highway and natural resources dimension is one to which we shall return.

public policy formation is Jack Walker's research on the diffusion of innovations.[4]

Walker, who examined the dates of adoption of 88 programs in 12 subject-matter areas in each of the contiguous American states, was able to rank the states in terms of the relative speed of adoption of those programs. Our intent here is to expand on his research *and* to relate the findings to the state expenditure studies.

Walker contends that "state officials make most of their decisions by analogy."[5] This assertion rests on studies of organizational decision making which stress that administrators have neither the time nor the energy to examine thoroughly the alternatives to complex problems. Rather, the administrator looks for the successful resolution of a similar problem elsewhere. State officials, therefore, may take many of their cues for program development from other states. Walker provides an extended discussion in support of this basic premise—i.e., policy is often made by emulation and analogy—and little would be served by replicating it here.

If administrators frequently make policy by analogy, it is important to know from whom they take their policy cues. In attempting to discover patterns of emulation, Walker reported the responses of 235 upper-level administrators in ten states to the following questions: which states are the best sources of information; which states usually seek information; and which states the administrator felt had the best departmental programs. The findings suggest the existence of a "national league," composed of California, New York, Wisconsin, Michigan, Illinois, and Minnesota, from which most other states seek information.[6] In addition, Walker reported extensive regional interaction and exchange; administrators tend to contact and be contacted by officials in neighboring states.

One aspect of Walker's research requires further discussion. He found the speed of diffusion of innovation among the states to be increasing: i.e., the elapsed time before an innovative program was adopted by at least twenty states has decreased in recent years.[7] His explanation for this quickening of diffusion is the development of specialized communication networks among public officials. In addition to the better known organizations, such as the Council of State Governments and the Federal Commission on Intergovernmental Relations, there exist more than 80 national, professional associations of state officials—e.g., the National Association of State Highway Officials; the National Association of State Supervisors and Directors of Secondary Education; and the Association of State and Territorial Health Officers. Most of these specialized groups meet regularly and publish journals or newsletters. They facilitate the exchange of policy and program development information among the administrators in the fifty states.[8]

Thus, Walker's discussion implies that decision making by emulation or analogy occurs primarily within policy areas. The head of a state health agency may be expected to interact with and read about the activities of health officers in other states rather than to focus, for example, on the activities of highway department officials. In reporting his data, however, Walker did not distinguish the responses of the state administrators he interviewed within specific policy areas.

Our aim is to introduce into this discussion the differences in assessments of state programs *within policy areas.* It is less important, after all, to know that executives in Utah generally consider California and New York to be worthy of esteem than it is to know how much consensus there is among, for example, health officials about the best state health department in the country.

### Variation in Assessments of State Programs by Policy Area

The data were gathered by mail questionnaire in 1971–1972 from appointed, upper-level administrators in ten American states.[9] Included were state executives from California, Ohio, Wisconsin, Louisiana, New Mexico, Tennessee, Kansas, Montana, South Dakota, and Nevada.[10] The reader will note the absence of any New England or Middle Atlantic states in this sample. This may lead to fewer references to Northeastern states. Our concern, however, is not with detailing communication patterns, but rather with measuring, if only roughly, the extent and effects of agreement about the most esteemed state agency among state executives within general policy areas. And, as we shall see, the data suggest that where states have outstanding programs, administrators in other states are aware of them—e.g., among our respondents in Wildlife and Fisheries agencies, there is near unanimity that Missouri has the best programs.

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[4] This research is reported in Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review,* 63 (September, 1969), 880–899, and in "Innovation in State Politics," in Jacob and Vines, pp. 354–387. Subsequent references to Walker's research will refer to this latter work.

[5] Walker, pp. 364–368.

[6] Walker, p. 380.

[7] Walker, pp. 376–377.

[8] The development of communications networks may lead to increased decision making by emulation. This, in turn, may produce increased demand for additional channels of communication, further stimulating the process.

[9] We are engaged in a longitudinal study of state executives. The initial research, conducted in 1970, elicited 1,870 returns of a four-page mail questionnaire which is a response rate of 80 per cent. This research is described more fully in Fred W. Grupp, Jr. and Alan R. Richards, "Partisan Political Activity among American State Executives," paper delivered at the Annual Meeting of the American Political Science Association, 1971.

The research reported here is based on a second questionnaire mailed to the original 1,870 respondents. This mailing produced 907 usable returns. The reduced response rate is due, in part, to occupational mobility; some of the executives had retired or changed jobs. Perhaps even more relevant was the inclusion of a Thematic Apperception Test in the second questionnaire. The second-wave respondents differ only minutely from the first in objective characteristics—i.e., they are very similar in terms of age, sex, race, education, and type of agency employment. In short, they appear to be a representative subset of the original respondents.

Other differences between Walker's research and our own require discussion. First, our sample included the high-ranking appointed officials of *all* agencies administering statewide programs within the ten states. The major policy areas included in this study, but not in Walker's, include natural resources, business and economic regulation, health (only mental health officials are included in Walker's interviews) and public works agencies. The responses of advisory personnel, such as budget officials, legislative reference personnel, and governor's staff, constitute a large portion in Walker's sample.[11] The overrepresentation of these kinds of officials reflects Walker's great concern with the adoption of innovative and possibly controversial programs. The relevance of such advisory personnel is obvious, since new programs will be scrutinized for cost and political acceptability prior to adoption. Our interest is more general. We wish to identify those state agencies which are widely esteemed on the assumption that the innovative policies which interest Walker are perceived initially by practitioners rather than by advisory personnel, and that important program and policy changes may take place within agencies without attracting much attention. For example, the practice of "grooving" highways to reduce skidding on wet roads may spread throughout the country without much controversy or change in highway department budgets.

A second distinction concerns the measures which were used. Many of Walker's findings are based on reported communications between state officials, of the sort, "from which states have you sought information; which states have contacted you for information?" He also constructed an Index of Approval composed of responses to several questions about program quality in other states.[12] This latter approach most nearly resembles our own. Each administrator in our sample was asked to rate his or her own agency on a ten-point, self-anchoring scale in the form of a ladder with the top labeled "absolutely ideal" and the bottom labeled "the very worst." Subsesequently, the executives were directed to rate the "best state agency carrying out the same type of program" as their own, and then to identify the state. This produced assessments of the perceived quality of their own agency, identification of the state with the best agency, and a measure of the difference in quality between their agency and the one that they most highly esteem. For the purposes of this paper, we shall concentrate on the extent of agreement about the states having the most nearly ideal agencies.

The data presented in the first column of Table 1 indicate that two states, California and New York, are mentioned much more frequently than any of the others. Together they account for 34 per cent of the total citations. This finding is fully consistent with Walker's. What might not have been anticipated on the basis of his research are the high levels of esteem accorded Texas, Oregon, Washington, Colorado, and Florida.[13] According

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The state executives are all appointed, politically or under a merit system, and serve in a state's executive branch. Excluded are elected officials; officials of state institutions of higher learning; members of professional and occupational licensing boards; executives in agencies with less than a statewide jurisdiction, such as a port authority; executives in state institutions, such as prisons or hospitals; and uniformed police officials. The goal was to include only upper-level appointed state executives *and* to include comparable executives in each of the ten states.

[10] The choice of states was influenced by Norman R. Luttbeg's study, "Classifying the American States: An Empirical Attempt to Identify Internal Variations," *Midwest Journal of Political Science,* 15 (November, 1971), 703–721. Four political "regions" emerged from his analysis: Industrial, with 17 states; Southern, with 17 states; Sparsely Populated, with 13 states; and Frontier, with 3 states. Our sample of states includes three from the Industrial Region (California, Wisconsin, and Ohio), three from the Southern Region (Louisiana, New Mexico, and Tennessee), three from the Sparsely Populated Region (Kansas, Montana, and South Dakota), and Nevada from the Frontier Region.

[11] Walker at p. 379 describes the respondents to his survey as "top administrators in the departments of education, labor, mental health, welfare, the budget bureau, legislative reference service, and the governor's chief assistant for program development."

[12] Walker, p. 381.

[13] Walker (pp. 378–385) deals with the persistence of regionalism in cue-taking among state administrators. Regional leaders, such as Texas and Florida, emerge here as well. Overall, 20 of the 48 contiguous states are included in the two studies. The combined results suggest the existence of national and regional leaders. Because of the nature of the sampling involved, however, only the rough outlines of the influence system are revealed. No definitive mapping of the system is claimed.

**Table 1. Number of Citations[a] as State with Best Agency by General Policy Area**

| | | Policy Area[c] | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| State Cited[b] | Total | Public Works | Health | Natural Resources | Business Regulation | Education | Human Relations | Welfare | Auxiliary | State Cited |
| Alabama | 1 | | 1 | | | | | | | Alabama |
| Alaska | 1 | | | | | | | 1 | | Alaska |
| Arizona | 4 | | | | | 1 | 2 | | 1 | Arizona |
| Arkansas | 3 | | | | | 1 | | 1 | 1 | Arkansas |
| California | 103 | 30 | 9 | 21 | 10 | 11 | 2 | 3 | 17 | California |
| Colorado | 15 | 3 | 1 | 2 | 1 | 4 | | 1 | 3 | Colorado |
| Connecticut | 4 | | 1 | 1 | | 1 | | 1 | | Connecticut |
| Florida | 19 | 3 | 1 | 6 | 1 | 4 | | 2 | 2 | Florida |
| Georgia | 7 | | 1 | 1 | 1 | 4 | | | | Georgia |
| Hawaii | 5 | 1 | | | 1 | 2 | | | 1 | Hawaii |
| Idaho | 1 | | | | | | | | 1 | Idaho |
| Illinois | 14 | 2 | | 5 | 4 | 3 | | | | Illinois |
| Indiana | 2 | | 1 | | | | 1 | | | Indiana |
| Iowa | 5 | 2 | | | | 1 | | 1 | 1 | Iowa |
| Kentucky | 7 | 1 | 1 | 2 | | | 1 | | 2 | Kentucky |
| Maine | 1 | | | | | | | 1 | | Maine |
| Maryland | 4 | | 1 | | 1 | | 1 | | 1 | Maryland |
| Massachusetts | 5 | | | | 2 | 1 | | 2 | | Massachusetts |
| Michigan | 30 | 1 | 7 | 6 | 2 | | 2 | 3 | 9 | Michigan |
| Minnesota | 18 | 1 | 4 | | | 1 | 1 | 8 | 3 | Minnesota |
| Missouri | 10 | | 1 | 7 | 1 | | | 1 | | Missouri |
| Montana | 1 | | | 1 | | | | | | Montana |
| Nebraska | 3 | | 1 | 1 | | | | | 1 | Nebraska |
| Nevada | 5 | 1 | | 1 | | | 1 | 1 | 1 | Nevada |
| New Hampshire | 1 | | | | | 1 | | | | New Hampshire |
| New Jersey | 5 | | | | 2 | 1 | | | 2 | New Jersey |
| New York | 65 | 2 | 19 | 1 | 11 | 10 | 9 | 3 | 10 | New York |
| North Carolina | 11 | | 2 | 2 | 1 | 1 | 1 | | 4 | North Carolina |
| North Dakota | 6 | 1 | | 2 | | | | 3 | | North Dakota |
| Ohio | 14 | | | 1 | 2 | 8 | | | 3 | Ohio |
| Oklahoma | 6 | | 1 | 1 | 2 | 1 | | 1 | | Oklahoma |
| Oregon | 20 | 1 | 2 | 4 | | 3 | 2 | 3 | 5 | Oregon |
| Pennsylvania | 7 | | | 2 | 1 | 2 | | 1 | 1 | Pennsylvania |
| Tennessee | 1 | | | | | 1 | | | | Tennessee |
| Texas | 30 | 7 | | 1 | 3 | 9 | 2 | 4 | 4 | Texas |
| Utah | 10 | | | 1 | 1 | 2 | 5 | | 1 | Utah |
| Vermont | 1 | | | | | | | 1 | | Vermont |
| Virginia | 3 | | 1 | 1 | 1 | | | | | Virginia |
| Washington | 21 | 1 | | 6 | | 5 | 1 | 3 | 5 | Washington |
| West Virginia | 2 | | | | | | 1 | 1 | | West Virginia |
| Wisconsin | 24 | 2 | 1 | 2 | 4 | 2 | 2 | 6 | 5 | Wisconsin |
| Totals | 495 | 59 | 56 | 78 | 52 | 80 | 34 | 52 | 84 | Totals |

[a] Of the 907 respondents, 158 did not name the state with the "best agency carrying out the same type program." In addition, the 242 executives who named their own state as having the best agency were excluded since they are not likely to seek policy guidance from other states. Finally, the responses of 12 executives in civil defense or statewide cultural agencies were also deleted since those categories contained too few cases for analysis. After all exclusions, 495 responses remain.

[b] Nine states received no citations and, therefore, are not listed in the left-hand column of the table. These states are: Delaware; Kansas; Louisiana; Mississippi; New Mexico; Rhode Island; South Carolina; South Dakota; and, Wyoming.

[c] The agency coding within policy areas is presented in the Appendix.

to the data in Table 1, it is clear that esteem is distributed unequally among the states. Nine states received no citations at all, while an additional eight were mentioned as having the best state agency only once. Viewed another way, if esteem were distributed equally, each state would have received almost 10 of the 495 total citations. In fact, only 15 (30 per cent) of the states were mentioned that frequently.

There are also great variations in the patterns of distribution of a state's esteem across policy areas as shown in the rows of Table 1. For example, only California, New York, and Wisconsin received favorable mentions in all eight categories of agency type. Some states, such as Nevada, North Carolina, and Oklahoma, tended to have evenly distributed citations across a relatively large number of policy areas. In contrast, other states (Missouri, Minnesota, and Illinois are good examples) received a large proportion of their favorable citations in one or two policy areas.

More important are the variations in the distribution of esteem within policy areas as shown in the columns of Table 1. The extent to which esteem in a particular policy area is distributed unequally among the cited states is a measure of consensus about the best state agency. Where proportionately greater numbers of states are cited or where the citations as best state agency are distributed more evenly, consensus is lacking and less agreement should exist on policy and programs worthy of incorporation. No policy area displayed in Table 1 is characterized by total consensus (where one state received all the citations) or by total dissensus (where each of the states are cited equally). Yet there are some obvious differences in the patterns of responses within policy areas: some tend toward consensus, others toward dissensus. For example, more than half (30 of 59) of the upper-level state executives employed in public works departments cited California as the state with the best public works agency. In contrast, no state received more than 14 per cent (11 of 80) of the citations of education officials.

One way to measure the tendency toward consensus within policy areas would be to calculate the percentage of esteem awarded to the most esteemed state and cumulate the per cent obtained as the second most esteemed state is added, and so on. This procedure is cumbersome, however, and the results, while suggestive, do not lend themselves to direct comparison. Fortunately, economists have developed better ways of displaying and comparing inequalities in the distribution of resources, such as income. They frequently utilize the Lorenz curve to display the extent of income inequality within a system and utilize Gini coefficients to compare the extent of inequality between systems. The Lorenz curve displays the cumulative proportions of the resource or attribute (in this case, esteem) on the vertical or $y$-axis accruing to cumulative proportions of the population (in this case, states) from the least to the most esteemed on the horizontal or $x$-axis. Perfect equality in the distribution of an attribute is represented in a Lorenz figure by a diagonal straight line from the lower left-hand corner to the upper right-hand corner. The extent of inequality is indicated by the degree of "convexity" of the Lorenz curve. The Gini coefficient is obtained by dividing the area between the diagonal line and the Lorenz curve by the total area under the diagonal line. Gini coefficients range from +1.00 (perfect inequality) to 0.00 (perfect equality). Thus, the larger the Gini coefficient, the more unequal the distribution of esteem and the greater the consensus about the better state agencies.

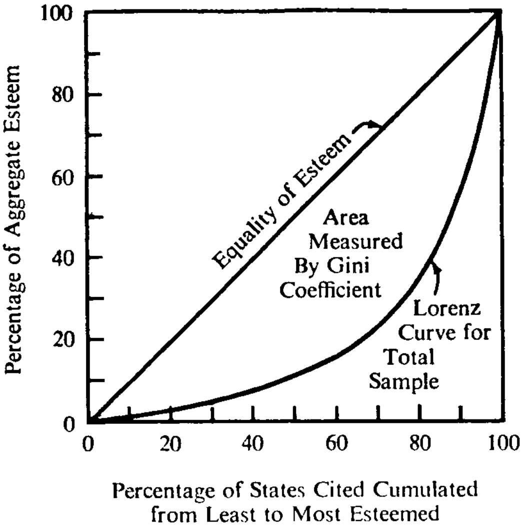

Figure 1. The Lorenz Curve of Inequality of Esteem

A Lorenz curve of inequality of esteem is presented in Figure 1. This curve is based on the overall distribution of esteem (495 citations) among the 41 states listed in Table 1. The Gini coefficient for this curve is .603.[14]

[14] Gini ratios are finding increased use in political science. See, among others, Bruce M. Russet, "Inequality and Instability: The Relation of Land Tenure to Politics," *World Politics*, 16 (April, 1964), 442–454; Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965), chap. 3; and Thomas R. Dye, "Income Inequality and American State Politics," *American Political Science Review*, 63 (March, 1969), 157–162.

The formula utilized here in calculating Gini coefficients is described in Herman P. Miller, *Rich Man, Poor Man* (New York: Thomas Y. Crowell, 1971), pp. 274–276.

**Table 2. Inequality of Esteem by Policy Area, in Rank Order**

| Policy Area | Gini coefficient |
|---|---|
| Public Works | .602 |
| Health | .554 |
| Natural Resources | .506 |
| Business Regulation | .456 |
| Education | .433 |
| Human Relations | .383 |
| Welfare | .374 |
| Auxiliary | .367 |

Of interest is the extent to which the Gini coefficients vary between policy areas. It will be remembered that the larger the coefficient, the greater the inequality of esteem. A large Gini coefficient, therefore, indicates that a small percentage of states received a large percentage of the favorable citations; there is general agreement about the better state agencies. A small coefficient indicates the absence of any such consensus. The Gini coefficient for each of the policy areas is presented in Table 2. Inequality of esteem is greatest in public works agencies.[15]

A brief discussion of the variations in Gini coefficients between policy areas will lay the groundwork for linking those results with policy making in the American states. To begin, let us examine the probable causes of the differences in Gini ratios displayed in Table 2. A most apparent difference between agency types characterized by higher levels of consensus about the better state agencies and those with lower levels of consensus is the extent to which their programs generate class-based conflict. By and large, the policy areas ranked high in Table 2—public works, health, natural resources—are not central to the "have/have-not" dimension of politics.

In addition to differences in clientele, the agency types for which inequality of esteem is greatest also have program outputs which are more easily evaluated. Either they are tangible, such as roads or boat-docking facilities, or they lend themselves to easy comparison, such as infant mortality statistics. It is noteworthy that public works agencies, where consensus about the better state programs is greatest, are characterized by both few organized clients and tangible outputs. Education, welfare, and human relations agencies, on the other hand, are more frequently involved in class-based conflict, deal directly with larger publics, and have less clearly defined standards for evaluating program effectiveness.

Thus far, all that can be said with assurance is that levels of consensus about the best state agency vary by policy areas among upper-level state executives. While this is an important modification in the diffusion-of-innovation literature,[16] it contributes only indirectly to our understanding of public policy making in the American states. Far more precise measures of the relative influence of various political actors, including state administrators, are needed before the public policy process will be understood.

## Inequality of Esteem and Political Influence

Is there a relationship between an administrator's ability to point to a successful program in another state and his relative influence in his state's policy-making process? Walker points out that "uncertainty and the fear of unanticipated consequences have always been formidable barriers to reform. Proponents of new programs have always had to combat the arguments of those who predict dire consequences if some innovation is adopted. . . . Inertia can more easily be overcome if the proponent of change can point to the successful implementation of his program in a similar setting."[17]

We hypothesize that there is a relative increase in the policy-making influence of those administrators who are able to cite effective programs in other states. One difficulty in testing the hypothesis with these data is that each of the executives (Table 1) did, in fact, name another state as having the more nearly ideal agency. Thus, each of the administrators should receive an equal increment of additional influence.

This is not a likely result, however, since in some policy areas there is little agreement about which state's programs are effective and worthy of emulation. Rather, we assume that the greater the consensus among executives about the leading state agencies, the more likely that policy recommendations which cite program innovations from those states will be received credibly by policymaking actors, such as the governor, legislators, and the like.

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[15] While Gini coefficients are frequently directly compared to one another, there are biases which may lead to distortions in the coefficients if the data are aggregated on the basis of unequal numbers of cells or if a large percentage of the cases fall in one cell. See Richard A. Genson, "Gini Ratios: Some Considerations Affecting Their Interpretation," *American Journal of Agricultural Economics* (August, 1970), 444–447.

The Gini coefficients computed here are subject to these sources of distortion. In each instance, the coefficients were calculated on the number of states receiving citations, rather than on a base of 50 states. The rationale for this procedure is that our interest lies in the extent to which esteem is distributed unevenly among those states which are cited. When the ratios are computed on the basis of 50 states, the effect is to increase all of the coefficients to the .8 and .9 range, and the data are not as revealing. What is important is that the rank ordering displayed in Table 2 is not changed by the alternate computation method.

[16] See also Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review*, 67 (December, 1973), 1174–1185. Professor Gray makes a similar point: diffusion of innovation varies by policy area.

[17] Walker, p. 366.

In contrast, in welfare departments, for example, where there is little agreement about any state having programs worth emulating, the executive has less to offer the other policy makers. The executive cannot provide a proven solution to the problem and, therefore, will be less influential in the decision-making process. Thus, our revised hypothesis is: *The greater the inequality of esteem in a policy area, the greater the policy-making influence of administrators in that policy area.* Policy areas were ranked by inequality of esteem in Table 2. According to our hypothesis, public works officials should be relatively more influential than executives in agencies characterized by less consensus about the leading state programs.

Assessments of policy-making influence were gathered from the respondents by means of three openended questions. The executives were asked to list in order of importance the "three agencies, groups, or individuals (such as governor, legislators, etc.) that are the most influential in determining your agency's (a) Broad general *policy;* (b) Specific *programs* implementing policy; and, (c) Individual *decisions* implementing programs." These questions elicited a wide variety of responses, ranging from powerful private individuals and groups to federal agencies and the U.S. Supreme Court.

Reported in Table 3 are the percentages of executives within policy areas naming their own agency or agency personnel as the most influential actor(s) at each of the three policy-making levels. Since we are dealing with perceptions of influence, it is appropriate to distinguish the responses of the most powerful executives from the responses of those further down in the hierarchy. It should be remembered that *all* of the respondents are high-level executives employed in important positions in their states. The distinction we make is between agency heads and other executives at the top of the agency's organization chart and those executives who are lower in the hierarchy by one level or more.

In addition to percentages, Spearman's *rho* between the rank ordering of levels of consensus (Table 2) and ordering of percentages of executives naming their own agency as most influential in the policy-making process is presented for each of the three policy levels. According to our hypothesis, the *rho*'s should be positive; that is, as consensus about best state programs increases, the percentage of state executives perceiving their own agency as most influential should increase.

The rank-order correlation statistics presented in Table 3 are uniformly positive and reach statistical significance at each policy level among the very highest level executives.[18] We are less impressed by the statistical significance than by the relative consistency of the findings when level of organization is controlled. The fact that the predicted relationship between rank orderings of policy areas by inequality of esteem and percentage citing own agency as most influential persists—though short of statistical significance—among the more junior executives, strikes us as additional support for the hypothesis.[19]

Consideration of other responses to the three policy questions (not reported here) indicate that the executives understood what was being asked of them and answered in realistic ways. For example, executives in agencies receiving grant-in-aid funds more frequently cited federal agencies as influential than did those in agencies receiving little or no federal funds. Education officials perceived local governmental units as influential, while welfare, human relations, and business regulation administrators were mindful of interest groups and their clientele. Furthermore, the patterns of responses make sense. As the policy level becomes less general and more specific—i.e., across the columns of Table 3—the percentages of administrators identifying their agency as most influential increases. In short, we believe that the executives' responses are valid and reliable, *at least from their point of view.* This does not mean that their perceptions are not distorted, nor does it determine whether the assessments of top-level executives are more accurate than those of executives lower down in the organizational hierarchy. The data at hand do not permit distinctinctions of this kind, and the policy-making process is so little understood that we cannot either affirm or dismiss the executives' view of

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[18] It might be argued that the inclusion of the heterogeneous auxiliary agencies artificially inflates the *rho's.* The respective rank-order correlations (*rho*) with auxiliary agencies excluded are: .555; .439; .505; .378; .714; and, .392. The critical value ($p$. .05) for $N$ of seven (one-tailed test) is .714.

[19] An alternate test of the hypothesis, suggested by one of the anonymous reviewers, is to compare the perceptions of agency influence at each of the three levels of policy making between those executives (type I) who named either of the two most esteemed states as having the more nearly ideal agency and those of the executives (type II) who cited a less esteemed state. This analysis was performed. The type I executives were more likely to perceive their agency as most influential in the policy-making process at each of the three levels of decision making—General policy, 24% to 19%; Specific program, 61% to 56%; and Individual decision, 84% to 79%. Each of these results is in the direction predicted by the hypothesis, though none of the percentage differences reaches the .05 level of statistical significance.

Table 3. Executives Identifying Their Own Agency as Most Influential by Policy Level, Organizational Position and Agency Type, in Per Cent

| | Policy Level | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | General Policy | | Specific Program | | Individual Decision | | Minimum (*N*) | |
| Agency Type | I[a] | II[b] | I[a] | II[b] | I[a] | II[b] | I[a] | II[b] |
| Public Works | 31 | 21 | 67 | 58 | 90 | 81 | (17) | (31) |
| Health | 43 | 27 | 86 | 63 | 93 | 87 | (16) | (61) |
| Natural Resources | 32 | 26 | 57 | 70 | 89 | 90 | (58) | (78) |
| Business Regulation | 13 | 19 | 64 | 53 | 73 | 74 | (24) | (30) |
| Education | 28 | 29 | 76 | 64 | 88 | 80 | (19) | (70) |
| Human Relations | 27 | 18 | 46 | 41 | 75 | 73 | (27) | (22) |
| Welfare | 16 | 16 | 59 | 55 | 87 | 85 | (19) | (54) |
| Auxiliary | 15 | 17 | 39 | 48 | 74 | 79 | (60) | (53) |
| (*N*)[c] | (224) | (411) | (221) | (406) | (220) | (399) | | |
| Spearman's *rho* | .690[d] | .629 | .666[d] | .512 | .713[d] | .429 | | |

[a] Agency heads and other highest level executives.

[b] Executives one level or more down in the organizational hierarchy.

[c] The 242 executives who named their own state as having the most nearly ideal agency are included here to maintain cell sizes. Of the 717 executives, 82 did not respond to the policy questions. The maximum *N* for this table is 635.

[d] $p < .05$. The critical value for an *N* of eight (one-tailed test) is .643.

power relationships in state policy making. Yet there *is* a relationship between the executives' perceptions of state agencies worthy of emulation and their perceptions of the influence of their own agency *in state public-policy formation and administration.*

## Evidence from the State Expenditure Studies

The state expenditure studies also indicate that the relative influence of actors in the policy-making process in the American states varies by policy area.[20] To what extent do those findings support our hypothesis that administrators in policy areas characterized by high levels of inequality of esteem are relatively more influential than are executives in agencies where there is less consensus about the leading state programs?

The most directly relevant and, in many ways, the most impressive of the policy output studies is that in which Sharkansky and Hofferbert factor analyzed 21 socioeconomic variables, 53 political system variables, and 34 state expenditure and public service variables. From this factor analysis emerged two environmental factors (industrialization and cultural enrichment); two political factors (competition-turnout and professionalism-local reliance); and three policy factors (welfare-education, highways-natural resources, and public safety). Their methodology is noteworthy since it avoids some of the problems associated with state spending studies by incorporating additional nonfiscal measures of policy output. For example, with regard to education the authors included the percentage of 1959 ninth-grade students who actually graduated three years later and also the percentage of students passing the Selective Service mental exam. Concerning highways they included a measure of highway safety (population per highway fatality) and measures of the numbers of miles of types of highways in relation to the population. In each instance, the attempt was made to utilize some measures of the actual service rendered to the state's population rather than relying solely on spending as an indicator of state public policy.

Sharkansky and Hofferbert find that political factors correlate more closely with the welfare-education dimension of state policy than with the highways-natural resources dimension. This finding supports our hypothesis.[21] Where consensus among state executives about the better state programs is low, as it is in welfare and education agencies (Table 2), the administrators can be expected to be relatively less influential in the policy-making process. Other political actors, such as political parties, interest groups, and elected officials, may be more influential. On the other hand, in those policy areas characterized by high consensus, such as highways (Public Works) and natural resources, the administrators may be expected to press for policies perceived as successful in other states, thus reducing in importance other influences such as the "competition-turnout" factor.

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[20] Cnudde and McCrone, p. 865. See also Sharkansky and Hofferbert, note 3 above. But compare Thomas R. Dye, *Understanding Public Policy* (Englewood Cliffs, N.J.: Prentice-Hall, 1972), p. 261 where he finds environmental factors (income, education, urbanization, and federal aid) are predominant.

[21] For those interested in the origins of an hypothesis, the actual evolution began with the Sharkansky and Hofferbert findings. How might one account for the waxing and waning of political factors as policy determinants? The serendipitous juxtaposition of Walker's research in the same issue of the *American Political Science Review* (September, 1969) suggested a possible explanation.

## Summary and Conclusion

Following Walker, we have assumed that executives in large complex organizations frequently make decisions by analogy and emulation. Administrators in the fifty states are linked by both formal and informal communication networks. They are increasingly aware of programs and policies, both in force and under consideration, in other states. These information sources provide the executive with a wide range of possible program solutions to similar problems in his own state. We further assume that a state executive is more likely to take policy cues from state agencies which are reputed to administer effective programs than from those he considers less worthy.

Our research not only substantiates Walker's conclusions about the existence of a set of highly esteemed states, but also expands upon his research by revealing variations in the patterns of esteem dependent upon the policy area under consideration. In addition, we hypothesized that the amount of policy-making influence accorded to state administrators would be directly related to their ability to provide proven solutions to problems in their state. In other words, executives in those policy areas characterized by high levels of consensus about the better state programs have their influence increased because they are able to approach legislators or the governor's staff with specific policy proposals which are known to be working well in those other highly esteemed states. This advantage does not accrue to executives in policy areas where there are no acknowledged leaders.

We drew upon the perceptions of agency influence cited by the state executives in our sample and upon published research on state spending in attempting to test the hypothesis. The evidence from the literature and from our own data provide some support for the hypothesis, though it cannot be considered conclusive. State administrators are only one of many influences in the policy process, albeit one that has received little attention. It was not our intent to ascribe to state executives an inordinate amount of influence; rather we believe that their ability to influence policy outcomes varies by policy area, within as yet uncharted limits.

Both the research on American state policy discussed here and the responses of the executives in our sample to questions about influential actors in the policy process indicate that the "mixes" of factors determining state policy outputs vary according to program. There is obvious need for better developed theory to account for the variations by policy area.

## Appendix

**Agency Type:**

*Education*—Department of Public Education (Instruction etc.), Central Agencies for Higher Education, and Vocational Education, Scholarship and Loan, and Educational Finance agencies, but *not* Educational Retirement agencies.

*Natural Resources*—Agricultural, Forestry, Mineral, Wildlife and Fisheries, Land, Water, Air, Conservation, Oil, Gas, and other Natural Resources agencies, and Parks and Recreation agencies.

*Business Regulation*—Commerce and Industry, Development and Planning (Tourist, Economic, Industrial), Transportation, Public Service and Public Utilities, Gambling and Alcohol (Sin), Insurance, Corporations, Banks, and Securities, Central Licensing, Consumers', and other Business and Economic Regulation agencies.

*Human Relations: Labor, Industrial and Special Groups*—Employment Security, (State Employment Service), Labor, Women's, Youth, Indian, Aged, Veterans', and other Human Relations agencies.

*Public Safety Agencies*—Civil Defense, Nuclear Energy, Fire Marshall, Disaster, and other Public Safety agencies.

*Health*—Public Health (General), Mental Health, Statewide Central Hospital or Institutional Health, and Public Health and Welfare (health division, professional line employees and all non-professional line health and welfare employees).

*Welfare*—Public or Social Welfare, Central Statewide Correctional Institutions, Rehabilitation, Probation and Parole, and Public Health and Welfare agencies (welfare division, professional line employees).

*Public Works*—Highway, Airport, Ports and Waterways, and other Public Works agencies.

*Auxiliary*—Immediate Office of the Governor, Department of Justice, Offices of Secretaries of State, Offices of Treasurers, Comptrollers and Auditors, Tax Revenue Collecting agencies, Budget and Finance agencies, Statewide Personnel agencies (Employee and Education Retirement included), State Building agencies, General Services Administration (Purchasing, Central Data Processing, Records Management, and other auxiliary agencies).

*Other*—Statewide Fair, Exhibition, Museum agencies, and other agencies.

# Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe*

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## Alliances and Balance of Power Systems

In this paper we partially test Professor Morton A. Kaplan's "theory" of the Balance of Power System first proposed in his well known *System and Process in International Politics*.[1] Given that nineteenth-century Europe comprised a balance of power international system and given that Kaplan has specified the "essential rules" of such a system, we ask the question: Did European alliance behavior in this period conform to Kaplan's theoretical expectations of a random alliance process?

The outbreak of World War I in 1914 traditionally marks the end of the nineteenth-century historical period. Conventional diplomatic wisdom suggests that war occurred because the balance of power system collapsed owing to a "hardening of alliances" some time after 1900. If a balance of power system looses its flexibility because the participants have abandoned systemic alliance formation rules, Kaplan's theory predicts such a result.[2] We therefore also ask the question: Why did a general European war break out in 1914?

We attempt to answer these related questions by applying probability theory, specifically a Poisson model, to the analysis of new data on fifty-five alliances among the five major European powers during the period 1814–1914. Because our research questions are so large-scale, our conclusions cannot be regarded as definitive. We do find, however, that the data examined very strongly support our hypotheses.

**Definitions.** No two concepts are more central to the analysis of international politics than "alliance" and "balance of power."[3] Theory in the field is so underdeveloped, however, that these concepts are usually vaguely defined, mixing description, prescription, and evaluation, so that an agreed-upon meaning for these terms does not at present exist.

Since we intend to test aspects of Morton Kaplan's theory of the behavior of balance of power systems, we shall use his definition of this concept. The pattern of interactions between two or more state actors, which composes a system of action, will be called a *balance of power system* if the following three conditions hold: (1) the system is without a political subsystem that authoritatively regulates the behavior of system members, such as a fully effective United Nations, (2) there are at least five essential state actors, and (3) the six rules of actor behavior specified by Kaplan are followed.[4] Kaplan regards nineteenth-century European international politics as one such balance of power system.[5]

Our conception of *alliances* is that they are a subclass of alignments wherein at least two states make a military commitment against at least one other state to fight or to remain neutral. Alliances share with alignments the following features: interstate cooperation or coordination over a prob-

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[1] New York: Wiley Science Editions, 1964. First published in 1957. All quotations of Kaplan in this paper are from the 1964 paperback reprint unless otherwise noted.

[2] Kaplan, *System and Process*, preface and pp. 27–29, 35–36.

[3] Representative discussions may be found in: George Liska, *Nations in Alliance: The Limits of Interdependence* (Baltimore: The Johns Hopkins University Press, 1962); Julian Friedman, Christopher Bladen and Steven Rosen, eds., *Alliance in International Politics* (Boston: Allyn and Bacon, 1970); Inis Claude, Jr., *Power and International Relations* (New York: Random House, 1962); Ernst B. Haas, "The Balance of Power: Prescription, Concept, or Propaganda," *World Politics*, 5 (July, 1953), 442–447 and "The Balance of Power as a Guide to Policy Making," *Journal of Politics*, 15 (August, 1953), 370–398; Kaplan, *System and Process;* and Paul Seabury, ed., *Balance of Power* (San Francisco: Chandler, 1965).

[4] Kaplan, *System and Process*, pp. 22–23. These rules are: "1. Act to increase capabilities, but negotiate rather than fight. 2. Fight rather than pass up an opportunity to increase capabilities. 3, Stop fighting rather than eliminate an essential national actor. 4. Act to oppose any coalition or single actor which tends to assume a position of predominance with respect to the rest of the system. 5. Act to constrain actors who subscribe to supranational organizing principles. 6. Permit defeated or constrained essential actors to re-enter the system as acceptable role partners or act to bring some previously inessential actor within the essential actor classification. Treat all essential actors as acceptable role partners."

[5] Kaplan, *System and Process*, pp. 22, 28–29.

lem; combination of state capabilities; pursuit of state interests; and mutual assistance. Alliances are distinct from alignments because: they have an actual or anticipated enemy (in Liska's terms—"Alliances are against, and only derivatively for, someone or something"[6]); they contemplate military engagement and the risk of war; and they are based upon mutual interest in the international *status quo* or its revision.[7] While alliances are often embodied in formal treaties, as part of the process of international politics, they can be informal and highly dynamic.

Most authorities recognize that alliances are basic to the operation of a balance of power system.[8] Kaplan argues that the six essential rules of the balance of power system operate to produce fluid alliances that "regulate" the system and prevent any one power or coalition of powers from achieving hegemony.[9] Indeed, the literature suggests that alliances are the primary means available to the foreign-policy maker in a balance of power situation; they enable leaders to maintain the pluralism of the system and to ensure the continued existence of its essential actors, albeit at the cost of solidarity. In a balance of power system, alliances are able to deter hegemonial ambitions by being fluid and flexible. Alliances are made without regard to ideology, cultural affinities, relations among monarchs and other elites; and in particular, each alliance is *independent of past alliances and alignment patterns.* Each is based solely upon present state interest and current threats to the balance of power.[10]

Behind this model of alliance process in balance of power systems is an assumption of actor rationality.[11] First, it is assumed that national interests have been ordered into a scale of preferences and, second, that decisions on when and with whom to form an alliance are based upon carefully considered cost-benefit analyses.[12] Thus, the process of alliance combination and recombination in a balance of power system resembles an infinite, ongoing, *n*-person, non-zero-sum game where the payoff remains indefinitely in the pot.[13]

**Theory.** In his study of *System and Process in International Politics*, Professor Kaplan produced a "heuristic" theory of six international systems that was meant to be explanatory, predictive, and prescriptive.[14] Kaplan's theory is a rational theory, for it only "predicts what state behavior will be if statesmen are rational, completely informed, and politically free to make external decisions on the basis of considerations of external rationality."[15] The theory is "heuristic" or general[16] for two reasons; first, the paucity of comparable and coded historical data made precise predictions "incautious" in 1957,[17] and second, a theory of international politics should not be expected to predict individual events. It should, however, be able to predict characteristic or modal behavior patterns.[18] Because in many respects Kaplan's theory represents a verbal formalization of traditional theorizing on international political behavior, his theory of the operation of a balance of power system is more specific than that of any of the other five types of system he analyzes. Moreover, Kaplan makes it explicit how his theory of balance of power politics can be tested: " . . . a high correlation between the pattern of national behavior and the essential rules of the international system would represent a confirmation of the predictions of the theory."[19] One does not have to share Professor Kaplan's positivism to be intrigued by the question of whether or not historical data fit his predictions about modal alliance behavior in a balance of power system.

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[6] Liska, *Nations in Alliance,* p. 12.

[7] Julian Friedman, "Alliance in International Politics," in *Alliance in International Politics,* ed. J. Friedman et al., pp. 4–5 and George Modelski, "The Study of Alliances: A Review," *Journal of Conflict Resolution,* 7 (December, 1963), 769–776 provide discussions of the salient characteristics of alliances.

[8] Representative discussions are: Kaplan, *System and Process,* pp. 35, 66, 115; Hans J. Morgenthau, *Politics Among Nations: The Struggle for Power and Peace,* 3rd ed. (New York: Knopf, 1963), pp. 167–223; and Friedman, "Alliance in International Politics," p. 23.

[9] Kaplan, *System and Process,* p. 125. See also Friedman, pp. 21–22 for the "functions" of alliances in international systems.

[10] These aspects of the role of alliances as a tool of foreign policy are further discussed in: Herbert Dinerstein, "The Transformation of Alliance Systems," *American Political Science Review,* 59 (September 1965), 589–601. See also: Liska, *Nations in Alliance,* p. 16; Kaplan, *System and Process,* p. 66; J. G. Cross, "Some Theoretic Characteristics of Economic and Political Coalitions," *Journal of Conflict Resolution,* 11 (June, 1967), 187; and Roger Masters, "A Multibloc Model of the International System," *American Political Science Review,* 55 (December, 1961), 788.

[11] This "model" is clearly prescriptive, and *hypothetically it is an accurate description and prediction of alliance politics in any balance of power system.* Like other rational models, it mixes description, explanation and possible prescription, a point well made by Otto Davis in "Notes on Strategy and Methodology for a Scientific Political Science," in *Mathematical Applications in Political Science IV,* ed. Joseph K. Bernd (Charlottesville: The University Press of Virginia, 1969), pp. 22–38.

[12] Friedman, "Alliance in International Politics," p. 23.

[13] Arthur Lee Burns, "From Balance to Deterrence: A Theoretical Analysis," *World Politics,* 9 (July, 1957), 495.

[14] Kaplan, *System and Process,* preface to the 1964 Wiley Science Edition.

[15] *Ibid.*

[16] Heuristic, of course, does not mean general; it means serving to discover. Professor Kaplan is not alone, however, in apparently equating the two terms.

[17] Kaplan, *System and Process,* 1964 preface.

[18] *Ibid.,* p. 24, and Morton A. Kaplan, "Some Problems in International Systems Research," in *International Political Communities: An Anthology* (Garden City: Doubleday Anchor Books, 1966), p. 471.

In such a system, Kaplan states, alliances are fluid and flexible, they are made for instrumental and not ideological reasons, and actors are indifferent about whom their alliance partners are.[20] These characteristics flow from the operation of the six essential rules of such a system, and they are similar to the views of many other authors on this topic. Kaplan's contribution lies in his prediction that, *when viewed from the perspective of the system*, alliances are equiprobable and time-independent: "the 'balance of power' system postulates that any alignment is as probable as any other alignment prior to a consideration of the specific interests which divide nations. Moreover, any particular alignment should not predispose the same nations to align themselves with each other at the next opportunity."[21] We read Professor Kaplan as saying that the alliance formation process in a balance of power system is a *stochastic process*. That is, in a balance of power system alliances occur from time to time, and these events over time are subject to probability laws because the past behavior of the alliance process has no influence on future behavior. If we have a process in which "the future is independent of the past" we have a purely random or stochastic process.[22] Kaplan specifically indicates that he is thinking in such probabilistic terms when he uses the metaphor of the behavior of molecules in a tank of gas to characterize balance of power alliance politics.[23]

**Hypotheses.** Now, there are many probability models of stochastic processes,[24] but of all processes that generate random variables, *this property of independence of past history is unique to the Poisson process*.[25] Poisson processes are also stationary stochastic processes, meaning that the origin of time is irrelevant.[26] This accords with Kaplan's theory because it implies that at whatever point in time new decision makers or new states enter the alliance process, they too are free of the past. Finally, in a balance of power system, alliances must be formed from time to time but it is predicted that they must not be so infrequent as hardly ever to occur or so frequent as to make rational calculations of state interest impossible.[27] Thus, alliances are *relatively rare events*, and it is a Poisson process that generates the distribution of such rare events over time.[28]

We therefore hypothesize, following Kaplan, that

H1: *in a balance of power international system, the occurrence of alliances will be stochastically distributed* (the number of alliances formed per unit of time is a Poisson random variable), and

H2: *in a balance of power international system, the time intervals between alliances are randomly distributed* (the distribution of interalliance intervals is a negative exponential random variable).

Because nineteenth-century Europe represents an empirical instance of a balance of power system, data on alliance behavior between 1814 and 1914 permit a direct empirical test of these two hypotheses inferred from Kaplan's theory.[29]

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[19] Kaplan, *System and Process*, p. 24.

[20] *Ibid.*, pp. 66, 115–116.

[21] *Ibid.*, preface.

[22] D. V. Lindley, *Introduction to Probability and Statistics from a Bayesian Viewpoint, Part I, Probability* (Cambridge: Cambridge University Press, 1965), p. 67.

[23] The metaphor, which comes in the middle of Kaplan's discussion of the operation of balance of power, is worth quoting in full. "Just as any particular molecule of gas in a gas tank may travel in any direction, depending upon accidental bumpings with other molecules, particular actions of national actors may depend upon chance or random conjunctions. Yet just as the general pattern of behavior of gas may represent its adjustment to pressure and temperature conditions within the tank, the set of actions of national actors may correspond to the essential rules of the system when the other variables take the appropriate specified values.

Thus, by shifting the focus of analysis from the particular event to the pattern of events, seemingly unique or accidental occurrences become part of a meaningful pattern of occurrences. In this way the historical loses its quality of uniqueness and is translated into the universal language of science" (*System and Process*, p. 25).

Professor Richard E. Hayes of C.A.C.I., Inc. disagrees with our reading of Kaplan's example, arguing in a personal communication that the metaphor refers to the limited predictability of social systems in general. Our disagreement illustrates the difficulties involved in deriving falsifiable hypotheses from verbal "theories" and "models" such as Kaplan's.

[24] An authoritative survey is given by William Feller, *An Introduction to Probability Theory and Its Applications*, 3rd ed. (New York: Wiley, 1968).

[25] Discussions of the Poisson distribution and the uniqueness of Poisson processes from a variety of perspectives are presented by: Howard Raiffa and Robert Schlaifer, *Applied Statistical Decision Theory* (Cambridge, Mass.: MIT Press, 1961), pp. 275–276; Oscar Kempthorne and Leroy Folks, *Probability, Statistics, and Data Analysis* (Ames: The Iowa State University Press, 1971), pp. 199–200; James S. Coleman, *Introduction to Mathematical Sociology* (Glencoe: The Free Press, 1964), pp. 288–289; Lindley, *Introduction to Probability and Statistics*, pp. 63–73; Richard E. Hayes, "Identifying and Measuring Changes in the Frequency of Event Data," *International Studies Quarterly*, 17 (December, 1973), 471–493.

[26] Lindley, *Introduction to Probability and Statistics*, p. 68.

[27] Kaplan, *System and Process*, p. 35.

[28] Karl Schuessler, *Analyzing Social Data: A Statistical Orientation* (New York and Boston: Houghton Mifflin Co., 1971), p. 412; Kempthorne and Folks, *Probability, Statistics and Data Analysis*, p. 91.

These two hypotheses about alliance behavior in balance of power systems can be thought of as characterizing the system while it is in an equilibrium state; that is, when it is operating according to Kaplan's six rules. Kaplan's theory is also concerned with the conditions under which one type of international system changes into another type. Conventionally, we think of the nineteenth-century European balance of power system as having broken down in World War I and as having been replaced by a new worldwide system featuring the League of Nations. Kaplan tries to identify factors that lead to changes in balance of power systems.

He identifies a number of "parameters" whose values, if they change, can lead to changes in system structure.[30] A key parameter is the "activity rate of a system," which is a cause of system flexibility.[31] An instance of this parameter in an empirical balance of power system would be *the rate of alliance formation* among essential system actors as measured over time for the system as a whole. The six essential rules of a balance of power system imply a pattern of fluid and moderately frequent alliances.[32] If these rules are violated, either by a rigid enmity, such as existed between France and Germany after the annexation of Alsace Lorraine, or by a decline in the systemic alliance formation rate,[33] then a loss of system flexibility will result and system-changing events are likely. We have set aside Kaplan's rigidity hypothesis for subsequent research and in this paper examine his prediction that

H3: *in a balance of power international system, a decline in the systemic rate of alliance formation precedes system changing events, such as general war.*

This hypothesis agrees with considerable speculation by historians about the origins of World War I and it can also be tested against data on nineteenth-century European alliance politics.

**Previous Research.** Published studies that empirically and systematically test propositions deduced from Professor Kaplan's heuristic theory are extremely rare. Several studies have used his concepts to describe historical interstate systems—Chi on the Chinese warlord system and Franke on the Italian city-state system[34]—but description is not hypothesis testing. One recent study by Healy and Stein does test Kaplan's rule 4 and part of his rule 6 against historical data on balance of power politics in the short interval 1870–1881. They find no support for Kaplan's rules, however.[35]

The study of historical alliance patterns has been conceptual and analytical, with few hypothesis tests.[36] Singer and Small, however, have made major contributions to our existential and correlational knowledge of historical alliances in their "Correlates of War" project.[37] The possibilities of theoretically informed research on alliances are illustrated in the recent volume by Holsti, Hopmann, and Sullivan[38] and in the most recent work of the "Correlates of War" project.[39]

There have been a few applications of stochastic models to international political phenomena, but only four that apply such models to alliance behavior. The first application of a Poisson model to international politics was that of Lewis Fry Richardson, which was extended by J. R. Moyal.[40] As discussed by Richardson, both authors demonstrated that Richardson's data on wars between 1820 and 1929 and Wright's data on wars between 1500 and 1931 were Poisson-distributed over time.[41] Much more recently, Singer and Small have demonstrated that the intervals between the outbreaks of all international wars and all interstate wars between 1816 and 1965 fit the negative exponential Poisson density function.[42] A further application of the Poisson model is Midlarsky's demonstration that coups in Latin America (1935–64) and Africa (1963–67) were Poisson-distributed over time.[43] Finally, another stochastic process model, Markov chains, has been applied to foreign conflict behavior 1955–1960,[44] and to the outbreak of World War I.[45]

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[29] G. U. Yule and M. G. Kendall, *An Introduction to the Theory of Statistics,* 14th rev. and enlarged ed. (New York: Hafner, 1950), p. 169.

[30] Kaplan, *System and Process,* preface and pp. 8, 35–36, 54–85.

[31] *Ibid.,* p. 74.

[32] *Ibid.,* pp. 35, 125.

[33] *Ibid.,* pp. 27–29, 74.

[34] Hsi-sheng Chi, "The Chinese Warlord System as An International System," pp. 405–425 and Winfried Franke, "The Italian City-State System as an International System," pp. 426–458, both in *New Approaches to International Relations,* ed. Morton A. Kaplan (New York: St. Martin's Press, 1968).

[35] Brian Healy and Arthur Stein, "The Balance of Power in International History: Theory and Reality," *Journal of Conflict Resolution,* 17 (March, 1973), 33–61.

[36] See Julian Friedman et al., eds., *Alliance in International Politics,* for a survey of this literature.

[37] J. David Singer and Melvin Small, "Formal Alliances, 1815–1939: A Quantitative Description," *Journal of Peace Research,* 3/1 (January 1966), 1–32; "Alliance Aggregation and the Onset of War, 1815–1945," in *Quantitative International Politics: Insights and Evidence,* ed. J. D. Singer (New York: The Free Press, 1968), pp. 245–286; "National Alliance Commitments and War Involvement, 1815–1945," *Peace Research Society (International) Papers,* 5 (1966), 109–140; and "Formal Alliances, 1816–1965: An Extension of the Basic Data," *Journal of Peace Research,* No. 3, (1969), 257–282.

[38] O. R. Holsti, P. T. Hopmann and J. D. Sullivan, *Unity and Disintegration in International Alliances: Comparative Studies* (New York: Wiley, 1973).

[39] Bruce Bueno de Mesquita and J. D. Singer, "Alliances, Capabilities, and War: A Review and Synthesis," in *Political Science Annual,* ed. C. P. Cotter (Indianapolis: Bobbs-Merrill, 1973), IV, 237–280 and Michael Wallace, "Alliance Polarization, Cross-Cutting, and International War, 1815–1964: A Measurement Procedure and Some Preliminary Evidence," *Journal of Conflict Resolution,* 17 (December, 1973), 575–604.

[40] Lewis Fry Richardson, "The Distribution of Wars in Time," *Journal of the Royal Statistical Society,* 107 (1945), 242–250; J. R. Moyal, "The Distribution of Wars in Time," *Journal of the Royal Statistical Society, Series A,* 112 (1949), 446–449.

The first application of a probability model to alliance behavior was by Horvath and Foster, who showed that Richardson's data on the size of wartime alliances fit a Yule distribution. This is consistent with the hypothesis that nations join alliances at a rate proportional to the number of nations in alliances of that size and that such alliances dissolve whenever a single member leaves.[46] Rood, who used the same data that we present in this paper, found that choice of alliance partner in nineteenth-century Europe was approximated by a probability model of random choice in voting bodies developed by Brams and O'Leary.[47] Brian Job has tested the hypothesis that alliance formation is a random process with a constant formation rate by applying a Poisson model to Singer and Small's data on 178 formal international alliances between 1815 and 1965. Despite imaginative treatment of the data, Job was unable to confirm his hypothesis except for all alliances, ententes and defense pacts between 1871 and 1914, and for all alliances and defense pacts from 1914 to 1939.[48] Finally, Siverson and Duncan have applied three different stochastic models (the Poisson, the contagious Poisson, and the Yule-Greenwood heterogeneity model) to the same Singer and Small alliance data set. Unlike Job's paper and this article, Siverson and Duncan do not present hypothesis tests because their objective was simply to examine long-run patterns in the initiation of alliance activity. They find that for the 1815–1914 period both the Poisson process model ($\chi^2=.76$, df$=1$, $p=.40$) and the contagious model ($\chi^2=.00$, df$=1$, $p>.99$) fit the data, a finding that contradicts Job.[49]

To our knowledge, these are the only applications of stochastic models, including the Poisson, to historical international political behavior. It is encouraging that certain central phenomena such as wars and the size of wartime coalitions can be described by stochastic process models. This suggests that other basic international behavior, such as alliances, may also be generated by such processes. It is discouraging to note the conflicting findings of Job and of Siverson and Duncan and the fact that except for the studies by Job, Rood, and Horvath and Foster, applications of stochastic models have not been related to hypothesis tests. Even in these three exceptions, the hypotheses tested were not inferred from an explicit theory of international politics. We therefore believe that this paper can break new ground in research on alliance behavior in balance of power systems and that it can make a contribution to positive theory building in international politics by testing three hypotheses inferred from Professor Kaplan's well known heuristic theory of system and process in international politics.

### Data Making

In order to evaluate our hypotheses, a data set on nineteenth-century European alliance behavior was created. The procedure used was Poisson sampling, which "consists of observing the process over a predetermined amount of time, length or other dimension, and counting the number of events which occur. . . ."[50] In Poisson sampling the time dimension ($t$) is predetermined, in the present case $t$ represents the one hundred and one year period from January 1814 to August 1914. On the other hand, in Poisson sampling the number of events ($x$) that occur is left to chance. In the present study $x$ represents an alliance between two or more core European powers and $\tilde{x}$ is the distribution of these fifty-five alliances across the $t$ dimension. Raiffa and Schlaifer prove that if the number of years preceding the $x$th alliance is $t$ or less, the conditional distribution of $\tilde{x}$ given $t$ and the intensity of the alliance formation process, $m$, is provided by the Poisson function:[51]

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[41] Lewis Fry Richardson, *Statistics of Deadly Quarrels* (Chicago: Quadrangle Books, 1960), pp. 128–142.

[42] J. David Singer and Melvin Small, *The Wages of War, 1816–1965: A Statistical Handbook* (New York: John Wiley, 1972), pp. 205–206.

[43] Manus Midlarsky, "Mathematical Models of Instability and a Theory of Diffusion," *International Studies Quarterly*, 14 (March, 1970), 60–84.

[44] Jonathan Wilkenfeld, "Models for the Analysis of Foreign Conflict Behavior of States," in *Peace, War and Numbers*, ed. B. M. Russett (Beverly Hills: Sage Publications, 1972), pp. 275–298 and Dina A. Zinnes and Jonathan Wilkenfeld, "An Analysis of Foreign Conflict Behavior of Nations," in *Comparative Foreign Policy*, ed. W. F. Hanrieder (New York: David McKay, 1971), pp. 167–213.

[45] Dina A. Zinnes, J. L. Zinnes and R. D. McClure, "Hostility in Diplomatic Communication: A Study of the 1914 Crisis," in *International Crises*, ed. C. F. Hermann (New York: The Free Press, 1972), pp. 139–162.

[46] W. J. Horvath and C. C. Foster, "Stochastic Models of War Alliances," *Journal of Conflict Resolution*, 7 (June, 1963), 110–116.

[47] Robert M. Rood, "Agreement in the International System," (Ph.D. dissertations in Political Science, Syracuse University, 1973); Steven J. Brams and Michael O'Leary, "An Axiomatic Model of Voting Bodies," *American Political Science Review*, 64 (June, 1970), 449–470.

[48] Brian Job, "Alliance Formation in the International System: The Application of the Poisson Model," a paper presented at the Annual Meeting of the International Studies Association, Americana Hotel, New York (March 13–17, 1973).

[49] Randolph M. Siverson and G. T. Duncan, "Stochastic Models of International Alliance Initiation, 1815–1965," Department of Political Science University of California, Davis, mimeo, n.d. [1973?], p. 12.

[50] Raiffa and Schlaifer, *Applied Statistical Decision Theory*, p. 283.

$$P\{\tilde{x} \mid t, m\} = f_P(x \mid m) = \frac{e^{-m}(m)^x}{x!}, \quad x = 0, 1, 2, \cdots \quad m > 0. \tag{1}$$

Thus, in undertaking a Poisson sampling data collection strategy, we have made it possible to analyze the distributional characteristics of our data by application of the well-known theoretical Poisson distribution and its variants.[52]

Our data-making strategy was similar to the events data approach,[53] but as applied to diplomatic histories rather than current events chronologies. A similar data-making strategy is being used by Rosecrance in his Situational Analysis Project at Cornell.[54] Following our detailed abstracting and coding rules,[55] we read nine authoritative and representative diplomatic histories and recorded each instance of alliance behavior reported by at least one historian to have occurred between January 1814 and August 1914.[56]

Operationally, the events ($x$) we call alliances are defined as all commitments, formal and informal, for the use or nonuse of military force (a) that were made between 1 January 1814 and 29 July 1914 by Great Britain, France, Prussia-Germany, Austria-Hungary, or Russia and at least one other power including those mentioned and (b) that had as their target the behavior *in Europe* of Great Britain, France, Prussia-Germany, Austria-Hungary, Russia or any grouping of these five states. Our sources usually reported formal commitments by citing a treaty, such as the 1834 Quadruple Alliance of Great Britain, France, Spain, and Portugal against Prussia, Russia, and Austria, who were joined by the Munchengratz Agreement of 1833. Informal alliances were most often embodied in the exchange of diplomatic notes or even verbal agreements by ambassadors to temporary joint fleet demonstrations such as that made in October, 1849, by Britain and France against Russia in support of Turkey.[57] Following this definition and our coding rules, fifty-five alliances were observed during the 1814–1914 time period. Twenty-eight of these alliances are of the formal type and are also part of the Singer and Small alliance data set.[58] The remaining twenty-seven are informal alliances and are unique to the present study. A complete listing of all fifty-five alliances is given by Rood.[59]

The reliability of our data collection operation cannot be assessed by the usual means of an intercoder reliability coefficient because all coding decisions were made by one author. Our data can be easily replicated, however, by consulting the methodological appendicies of Rood's dissertation.[60] The use of multiple sources was our strategy for achieving an acceptable level of reliability in the alliance counting process. Most of the fifty-five alliances, 74 per cent, are cited by two or more authorities. The average number of references per alliance is nearly three. Only fourteen alliances were mentioned by just one diplomatic historian. Finally, the nine histories we used were taken from the most frequently cited authorities used by the "Correlates of War" Project.[61] We therefore doubt that we have included in our data set an alliance that in fact did not occur in nineteenth-century Europe. We may have failed, however, to include some alliances, particularly the informal type, that did in fact take place between 1814 and 1914. This possibility can only be discounted after our data set has been independently replicated.

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[51] *Ibid.*

[52] The best discussion of social scientific applications of the Poisson distribution is given in J. S. Coleman, *Introduction to Mathematical Sociology*, pp. 288–380.

[53] Discussions of aspects of events data making are given in: Edward Azar, "Analysis of International Events," *Peace Research Reviews*, 4, No. 1 (1970); Edward Azar, R. A. Brody, and C. A. McClelland, *International Event Interaction Analysis: Some Research Considerations*, Sage Professional Paper in International Studies 02–001 (Beverly Hills and London: Sage Publications, 1972); C. F. Hermann, "What is a Foreign Policy Event?" pp. 295–321 in *Comparative Foreign Policy*, ed. Hanreider; P. M. Burgess and R. W. Lawton, *Indicators of International Behavior: An Assessment of Events Data Research*. Sage Professional Paper in International Studies 02–010 (Beverly Hills and London: Sage Publications, 1972); and P. J. McGowan, "A Bayesian Approach to the Problem of Events Data Validity," pp. 407–433 in *Comparing Foreign Policies*, ed. J. N. Rosenau (New York: Halsted Press (a Sage Publications Book), 1974).

[54] Brian Healy and Arthur Stein, "The Balance of Power in International History: Theory and Reality," Ronald Goodman, Jeff Hart, and Richard Rosecrance, "Testing International Relations Theory: Methods and Data in a Situational Analysis of International Politics," Ithaca: Cornell University Situational Analysis Project Paper No. 2, mimeo, January, 1970; and Jeff Hart, "Symmetry and Polarization in the European International System: 1870–1879," Ithaca: Cornell University Situational Analysis Project Paper No. 3, mimeo, (1972).

[55] As described in Rood, "Agreement in the International System," pp. 171–175.

[56] Our sources were: René Albrecht-Carrié, *A Diplomatic History of Europe Since the Congress of Vienna* (New York: Harper, 1958); E. V. Gulick, *Europe's Classical Balance of Power* (New York: W. W. Norton, 1955); H. A. Kissinger, *A World Restored: Metternich, Castlereagh and the Problems of Peace, 1812–22* (Boston: Houghton Mifflin, 1957); W. L. Langer, *European Alliances and Alignments, 1871–1890*, 2nd ed. (New York: Random House-Vintage Books, 1964); Andrei Lobanov-Rostovsky, *Russia and Europe 1825–1878* (Ann Arbor: George Whar Publishing, 1954); R. B. Mowat, *The European States System: A Study of International Relations*, 2nd ed. (London: Oxford University Press, 1929); L. C. B. Seaman, *From Vienna to Versailles* (New York: Harper, 1963); R. W. Seton-Watson, *Britain in Europe, 1789–1914* (New York: Howard Fertig, 1968); and A. J. P. Taylor, *The Struggle for Mastery in Europe 1848–1918* (Oxford: The Clarendon Press, 1954).

[57] Taylor, *The Struggle for Mastery in Europe*, pp. 2–3, 34–35.

[58] As described in Singer and Small, "Formal Alliances, 1815–1939," and "Formal Alliances, 1816–1965."

[59] Rood, "Agreement in the International System," pp. 183–201. This dissertation is available from University Microfilms in Ann Arbor, Michigan.

The validity of our data set for the purposes of this paper is enhanced by the fact that we ask only two simple questions of the data: (1) did an alliance commitment consistent with our operational definition occur? and (2) in what year did it occur? In this paper we do not engage in the difficult task of attempting to measure such things as the scope, level of commitment, or duration of alliances. The validity issue as it concerns this paper thus reduces to the question of whether or not our operational definition of alliance is suitable for the study of alliance behavior in balance of power systems.

Our assertion that nineteenth-century Europe was a balance of power international system is supported by the fact that thirteen out of fourteen scholars surveyed, including Morton A. Kaplan, claim that it was.[62] Only Rosecrance does not accord the term balance of power to the nineteenth-century European state system.[63] Our decision to study only the alliance formation behavior of the five greatest European powers is validated by the theoretical argument presented earlier that it is the essential or great powers that operate a balance of power system and that these five states were the major European powers of the nineteenth century.[64] Our focus on alliances directed at the European behavior of one or more of these five states derives from the fact that the balance of power system we are studying is the system of Europe from 1814 to 1914 and not the incipient worldwide system that emerged as a result of World War I. According to Singer and Small, extra-European powers achieved great-power status only very late in the 19th century—Japan in 1895 and the United States in 1899.[65] Finally, we have included informal as well as formal alliances because a focus on written commitments would exclude much of the nineteenth-century "balancing" behavior of Great Britain, a power regarded by many authorities as central to the operation of the European balance of power system,[66] and because there is nothing in Kaplan's theory to suggest otherwise.

Figure 1 presents our alliance formation data arrayed by five-year intervals for the entire nineteenth century. The first five years, which included the Congress of Vienna, saw the greatest amount of alliance formation by the five great powers. These relations held for the next period, when no new alliances were formed. In 1825 there began a fifty-year period of moderate levels of alliance formation that included the Crimean War and the unifications of Italy and Germany. The Bismarck era then saw a high level of alliance behavior that continued until 1909, the year of our last observed alliances. The five years preceding the outbreak of World War I in August 1914 saw no new alliances among the five great European powers. The mean rate of alliance formation by five-year periods is 2.75 with a moderate standard deviation of 1.94. As the figure suggests, formal and informal alliances co-occur. The product moment correlation coefficient ($r$) between the two types of alliances by five year period is 0.43 ($p \leq .05$ in a two-tailed test).

### The Poisson Distribution

We shall use the Poisson distribution in two distinct fashions in this paper. First, we shall compare the fit of our historical data to Poisson-derived distributions that predict the number of alliances formed over time and the intervals between alliances. Second, we shall use the Poisson

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[60] *Ibid.*, pp. 171–202.

[61] Singer and Small, "Formal Alliances, 1815–1939," pp. 24–27.

[62] Rood, "Agreement in the International System," pp. 64–67.

[63] R. N. Rosecrance, *Action and Reaction in World Politics* (Boston: Little, Brown, 1963), pp. 239–256.

[64] Supporting this argument are: Gulick, *Europe's Classical Balance of Power*, p. 4; Morgenthau, *Politics Among Nations*, p. 201; J. David Singer and Melvin Small, "National Alliance Commitments and War Involvement, 1815–1945," in *International Politics and Foreign Policy*, ed. J. N. Rosenau, 2nd ed. (New York: The Free Press, 1969), p. 515.

[65] Singer and Small, "National Alliance Commitments . . . ," p. 515 in the Rosenau reader.

[66] Albrecht-Carrié, *A Diplomatic History of Europe*, pp. 9–17; Taylor, *The Struggle for Mastery in Europe*, p. 54; Seton-Watson, *Britain in Europe*, p. 312; and Ludwig Dehio, *The Precarious Balance: Four Centuries of the European Power Struggle* (New York: Random House-Vintage Books, 1962), *passim.*

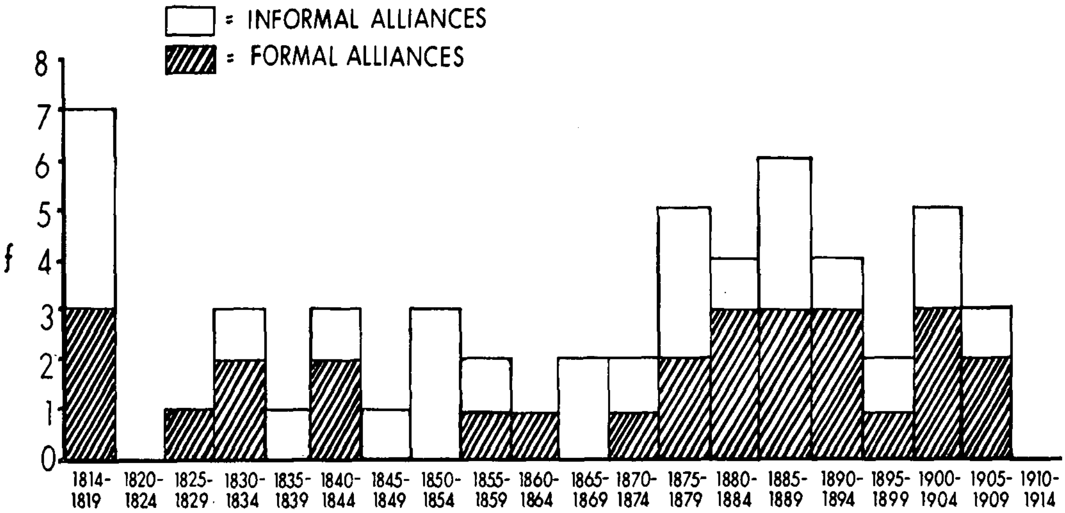

**Figure 1. Frequency Histogram of Alliance Formation in 19th Century Europe by Five-Year Period**

distribution to compute rates of alliance formation in nineteenth-century Europe.

The Poisson distribution, one of "a few distributions of great universality which occur in a surprisingly great variety of problems."[67] is discussed in most texts on probability and mathematical statistics.[68] A comprehensive treatment of the Poisson distribution is given by Haight, including many applications, and values of the distribution for given means ($m$) are tabulated by Kitagawa.[69] The applicability of the Poisson distribution and its variants to social science questions is extensively discussed with numerous examples by Coleman.[70]

Following Lindley,[71] the aspects of Poisson process theory relevant to our paper can be briefly outlined. If in a period of time $(0, t)A$ events occur and in a period $(t, t+h)B$ events occur and $A$ and $B$ are independent so that $p(B|A)=p(B)$, this not depending upon $t$, the process is said to be a purely random stationary process, or a *Poisson process*. In a Poisson process the following theorems hold:[72]

(1) the probability of no events in a fixed interval of length $t$ is—

$$p_0(t) = e^{-mt}, \quad \text{where} \quad m > 0 \tag{2}$$

(2) the density of time between any two events ($x_1$ and $x_2$) is given by—

$$f_1(x) = me^{-mx}, \quad \text{when} \quad x \geq 0 \tag{3}$$

(3) if $t$ is any fixed number, the probability of $x$ events in a fixed interval of length $t$ is—

$$p_x(t) = \frac{e^{-m}(m)^x}{x!} \tag{4}$$

(4) the expected number of events ($x$) in a time interval of length $t$ is $m$ (5)

(5) the expectation of time up to the $x$-th event, $\bar{t}_x$, is $x/m$ (6)

The key *parameter* in a Poisson process is seen to be $m$, which equals $\lambda t$ when $t$ is one. The proper physical interpretation of $m$, given theorem 4, is that it is the expected number of alliances per unit of time, i.e., the *rate* of formation. Conversely, from theorem 5, when $x=1$, the expected or average time between successive alliances is $1/m$. Theorem 2 states that the interalliance intervals are independently distributed negative exponential random variables. This theorem will be used to test H2: that in a balance of power system, the time interval between alliances is randomly distributed. Theorem 3 states that the number of alliances per unit of time is a Poisson random variable. This theorem will be used to test H1: that in a balance of power system, the occurrence of alliances will be randomly distributed.

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[67] Feller, *An Introduction to Probability Theory*, p. 156, who argues that the three principal distributions are the binomial, the normal, and the Poisson, a point agreed to by Yule and Kendall, *An Introduction to the Theory of Statistics*, p. 169.

[68] Feller, pp. 153–164; Lindley, *An Introduction to Probability and Statistics from a Bayesian Viewpoint*, pp. 63–74; William Mendenhal and Richard L. Scheaffer, *Mathmatical Statistics with Applications* (North Scituate, Mass.: Duxbury Press, 1973), pp. 81–85; Raiffa and Schlaifer, *Applied Statistical Decision Theory*, pp. 221–222, 275–289; and Yule and Kendall, pp. 189–194.

[69] F. A. Haight, *Handbook of the Poisson Distribution* (New York: Wiley, 1967); T. Kitagawa, *Tables of Poisson Distribution* (Tokyo: Baifukan, 1952).

[70] Coleman, *An Introduction to Mathematical Sociology*, pp. 288–311.

[71] Lindley, *An Introduction to Probability and Statistics*, pp. 63–73.

[72] We give only the theorems presented by Lindley relevant to this paper.

The standard statistical test for comparisons between Poisson-based predicted distributions and empirical alliance distributions is the Chi-square goodness of fit test as described by Pearson and Hartly and by Yule and Kendall.[73] The null hypothesis is that the observed distribution does not significantly depart from the expected Poisson-based distribution.[74] A second test of goodness of fit is based on the fact that in a Poisson distribution, $\sigma^2 = m$, the variance equals the mean, a unique feature of Poisson distributed data.[75]

In a recent article, Hayes has discussed applications of Poisson theory to the measurement of changes in the frequency of international political phenomena.[76] He presents a Poisson-based comparison technique that permits one to say, for example, how likely it is that eleven alliances occurred in the period $(t, t+h)$, given that just seven happened between 0 and $t$. This technique can be directly applied to test H3: that, in a balance of power system, a decline in the systemic rate of alliance formation precedes system-changing events, such as general war. In this application of Hayes's measurement routine to our data, one calculates the rate of alliance formation by specified time intervals over the century between 1814–1914 and then calculates the probability of observing $x$ alliances in each interval under the assumption that the observed $x$ represents a decline from the average rate $m$ (H1: $x < m$). These probabilities then become one's measures of balance of power system flexibility over time.

We conclude this discussion of the Poisson distribution by noting Coleman's belief that it is particularly appropriate for the analysis of sociopolitical phenomena because the Poisson distribution does not assume continuous level measurement; because a Poisson process occurs continuously over time rather than at discrete "trials" and thus is readily applied to naturally occurring events like alliances; and because the Poisson process is a rational model whose assumptions can mirror our assumptions about actual phenomena. Moreover, when its assumptions are met, a Poisson distribution is not an approximation to data; "it is the exact distribution and any other becomes an approximation."[77] We believe our research problem and our data fit the assumptions of the Poisson model, especially in that alliance formation in a balance of power international system is said by Kaplan to be based on rational calculations of costs and benefits.[78]

### Empirical Results

Let us assume that in a balance of power system that is in equilibrium the decision makers of each essential actor have a propensity to form alliances with and against other essential actors that is rationally designed to keep the system in equilibrium. Let this alliance propensity be denoted by $\alpha$ and let us additionally assume that each essential actor has about the same propensity. Then, if there are $N$ actors in the system, the systemic propensity to form alliances during a fixed period of time, $t$, will be $\alpha N$. When our data on alliance formation frequencies among the five great European powers between 1814 and 1914 are arrayed as in Table 1, $\alpha N = m = 0.545$. That is, one alliance was formed about every two years, or 0.545 alliances per year; this is the average rate of alliance formation in Europe in the nineteenth century when $t$ equals one year. Therefore, $\alpha = 0.545/5 = 0.109$, is our estimated actor propensity to form alliances. In applying the Poisson distribution to our data we therefore assume that $m$ is constant throughout the nineteenth century, and by implication since $N$ is constant, $\alpha$ was constant as well.

Table 1 represents our test of H1: that in a balance of power international system, the occurrence of alliances will be stochastically distributed over time. The evidence in the table strongly supports this hypothesis, for all alliances and for both formal and informal alliances. The Poisson expected frequencies are very close to observed frequencies; the Chi-square values indicate that in all three experiments more than 50 per cent to 90 per cent of comparable observations would show worse agreement; and the means and variances of the three empirical distributions are remarkably close. Table 1 indicates that formal and informal alliances are homogeneous, that they are part of the same alliance formation process. Moreover, since the alliances are Poisson distributed, we may conclude that they were generated by a Poisson process because "not only does

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[73] E. S. Pearson and H. O. Hartley, *Biometrika Tables for Statisticians, Volume I*, 3rd ed. (Cambridge: Cambridge University Press, 1966), pp. 11–12; Yule and Kendall, pp. 469–477.

[74] That is, only when $X^2$ is large, so that for a given degree of freedom its probability is less than .10 or .05, is the null hypothesis rejected and the inference made that the observed distribution was *not* generated by a Poisson process. This is not a very conservative procedure. It unfortunately leaves open the possibility of Type II error, i.e., the inference that the null hypothesis prevails when in fact the alternate hypothesis is correct.

[75] Kempthorne and Folks, *Probability, Statistics, and Data Analysis*, p. 92.

[76] Richard E. Hayes, "Identifying and Measuring changes in the Frequency of Event Data," *International Studies Quarterly*, 17 (December, 1973), 471–493.

[77] Coleman, *An Introduction to Mathematical Sociology*, pp. 291, 299.

[78] Kaplan, *System and Process*, 1964 preface.

**Table 1. The Formation of Alliances in Europe, 1814–1914, as a Poisson Distribution**

| Experiment[a] | Years with $x$ Alliances<br>$x=0$ | $x=1$ | $x=2$ | $x\geq 3$ | $\chi^{2b}$ | $p$ | $m$ | $s_x^2$ |
|---|---|---|---|---|---|---|---|---|
| All Alliances: | | | | | | | | |
| Observed $Nx$ | 61 | 31 | 6 | 3 | | | .545 | .501 |
| $Np(x; 0.545)$ | 58.6 | 31.9 | 8.7 | 1.8 | .338 | $>.50$ | | |
| "Formal" Alliances: | | | | | | | | |
| Observed $Nx$ | 77 | 21 | 2 | 1 | | | .277 | .299 |
| $Np(x; 0.277)$ | 76.6 | 21.2 | 2.9 | 0.3 | .019 | $>.80$ | | |
| "Informal" Alliances: | | | | | | | | |
| Observed $Nx$ | 78 | 21 | 3 | 0 | | | .267 | .256 |
| $Np(x; 0.267)$ | 77.3 | 20.6 | 2.8 | 0.3 | .012 | $>.90$ | | |

[a] Probabilities of the Poisson distribution, $p$ in $Np$, determined from T. Kitagawa, *Tables of Poisson Distribution* (Tokyo: Baifukan, 1952), pp. 13–14, 25.

[b] The degree of freedom for Chi Square in each test is one.

the process yield the distribution but the distribution, with mean proportional to the length of the interval, can only arise from the process."[79]

Table 2 presents our test of H2: that in a balance of power international system, the time intervals between alliances are randomly distributed. As the table indicates, the negative exponential Poisson distribution provides a good fit to the data when intervals of six months are the time units. The $\chi^2$ value of 4.74 does not permit us to reject the null hypothesis that the two distributions are similar. More than 30 per cent of comparable observations would be expected to show a worse fit. Hypothesis 2 is therefore confirmed. We note, however, that the observed frequencies of less than six months and between thirty-six and forty-two months are somewhat higher than expected. This tendency for alliances in nineteenth-century Europe to be formed quickly upon one another or with a lag of about three and one-half years deserves further study.

In order to test H3: that in a balance of power international system a decline in the systemic rate of alliance formation precedes system changing events, such as general war, we recoded our data into five-year periods as given in Figure 1. When so recoded, the number of five-year periods in which $x$ alliances occurred ($x\geq 0$), were also found to be Poisson distributed with a $\chi^2$ value of 1.11 with one degree of freedom, thus giving a $p>.20$. The closeness of fit between theory and observation is illustrated in Figure 2. This finding

**Table 2. Interalliance Intervals in Europe, 1814–1914, as a Poisson Distribution**

| Length of Time Between Alliances in Six Month Units | Observed Relative Frequency | Expected Relative Frequency |
|---|---|---|
| 1 | .315 | .222 |
| 2 | .148 | .174 |
| 3 | .093 | .135 |
| 4 | .074 | .105 |
| 5 | .074 | .082 |
| 6 | .056 | .064 |
| 7 | .167 | .050 |
| 8 | .000 | .038 |
| 9 | .018 | .030 |
| 10 | .000 | .023 |
| 11 | .018 | .018 |
| 12 | .000 | .014 |
| 13 | .000 | .011 |
| 14 | .018 | .009 |
| 15 | .000 | .007 |
| 16 | .000 | .006 |
| 17 | .000 | .004 |
| 18 | .018 | .004 |
| Totals | .999 | .996 |
| $\chi^2$ = 4.74, | d.f. = 4, | $p$ > .30 |

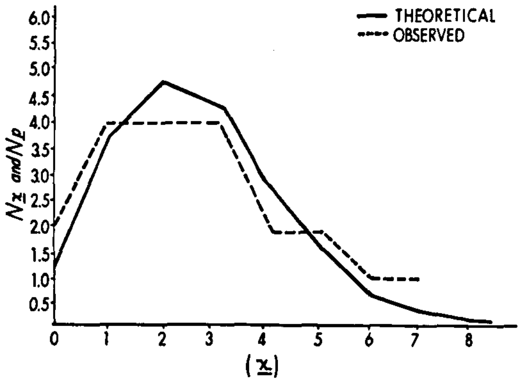

**Figure 2. Theoretical and Observed Distribution of Fifty-Five 19th Century European Alliances by Five-Year Period Under the Assumption that Alliance Formation is a Poisson Process ($m=2.75$)**

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[79] Lindley, *An Introduction to Probability and Statistics*, p. 70.

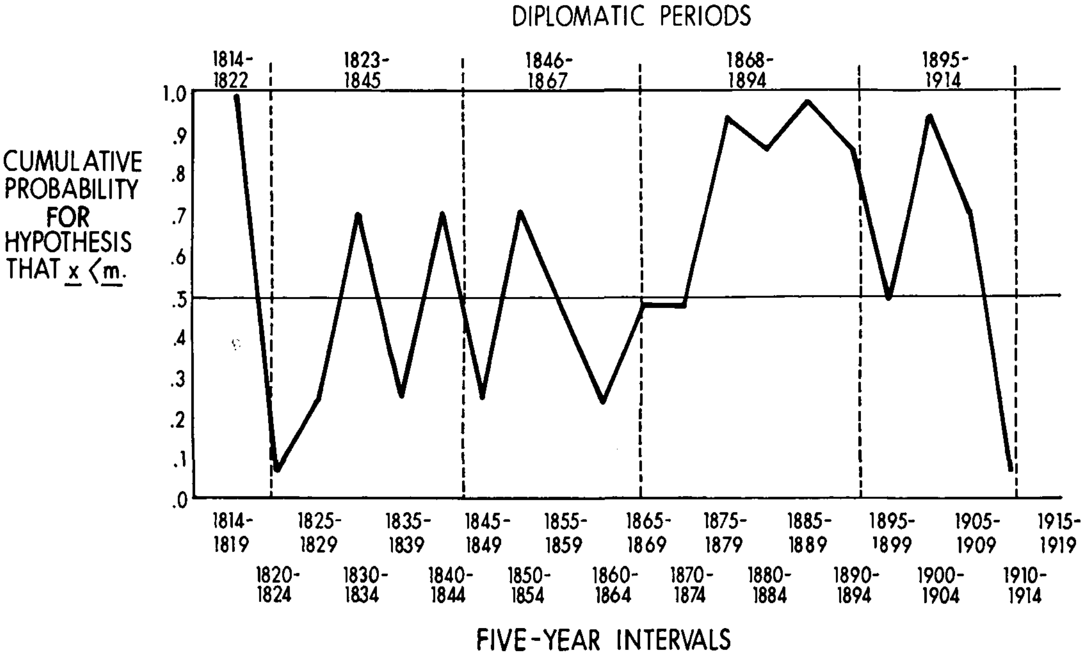

**Figure 3. Poisson Based Measures of Changes in the Rate of Alliance Formation in 19th Century Europe**

lends additional support to our confirmation of the first hypothesis.

Given that the five-year period average rate of alliance formation is $m = 2.75$, we can calculate the cumulative probability that the observed number of alliances formed in each period, $x$, is less than $m$. Taking our probabilities from Kitagawa,[80] we have plotted and joined them in Figure 3. The vertical axis of the figure represents the cumulative probability that $x < m$; as a probabilistic measure of changes in the rate of alliance formation it obviously ranges from zero to unity. Two horizontal axes are given in Figure 3. The lower one represents twenty periods of five-years length each. The upper time dimension divides the century into five diplomatic periods quite similar to Rosecrance's five nineteenth-century periods of multipolar concert (1814–22), quasi-polar truncated concert (1822–48), multipolar concert (1848–71), unipolar concert and alliances (1871–90), and bipolar concert and alliances (1890–1918)[81] but in fact based on Frank Denton's hostility cycles which indicate that war intensity was highest at each demarcation point.[82] Thus, the vertical dashed lines that separate the five periods represent the approximate points in time at which historical evidence suggests that the system changed to a new equilibrium state (Rosecrance) and at which quantitative data suggest war was most intense (Denton).

Figure 3 supports hypothesis 3, for the cumulative probability of observing no alliances being formed in the period 1910–1914, given that the century-long average activity rate of the system was 2.75, is only .064. This is the lowest measured alliance formation rate of the century except for 1820–1824, the quiet period after the burst of diplomatic activity that concluded the Napoleonic Wars. If we take the two diplomatic periods of 1868–1894 and 1895–1914 and calculate their five-year period activity rate, which is $m = 3.3$, the cumulative probability that $x < m$ in 1910–1914 is just .037. Whichever way we look at it, a clear-cut decline in system flexibility occurred after 1909, and this period immediately preceded an event that destroyed the European balance of power, perhaps forever.

While we would not want to push it too far, there appears to be a serendipitous finding of some theoretical interest in Figure 3. Note that in four out of five instances of change from one diplomatic period to another a decline in the alliance formation rate occurred. Only in the change from 1846/67 to 1868/94 is the change point not crossed by a negatively sloped line. These five points represent the times at which war was most intense (Denton) and when diplomatic historians point to changes in the structure of the system (Rosecrance). This finding would appear to lend further credence to our third hypothesis and it certainly merits further research.[83]

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[80] Kitagawa, *Tables of Poisson Distribution*, p. 65.

[81] Rosecrance, *Action and Reaction in World Politics*, pp. 232–266.

[82] F. H. Denton, "Some Regularities in International Conflict, 1820–1949," *Background*, 9 (February, 1966), 283–296. This journal is now the *International Studies Quarterly*.

### Interpretations and Conclusions

Our data strongly support our three hypotheses, and if these hypotheses have been reasonably inferred from Professor Kaplan's theory of the process of balance of power systems, we conclude that his theory has greater credibility than heretofore. The principal limitations of our study center on the fewness of the testable propositions we were able to derive from Kaplan, their high level of generality, and the simplicity of the data with which they were tested. Our research cannot be regarded as definitive on any of the questions it asks, but we would argue that our evidence is decisive with respect to our three hypotheses and that our paper overall presents a potentially fruitful research strategy for work on alliance behavior and international systems research.

Further research topics immediately suggest themselves. Obviously, our study should be replicated on other data sets on nineteenth-century Europe and extended to alliance behavior in other historical balance of power systems. The implications of Figure 3 about systemic change and alliance behavior and the clustering of interalliance intervals in Table 2 should be looked at. If it is true that alliances are generated by a Poisson-type process, then theoretical models that account for other Poisson-type processes, such as subatomic behavior and telephone exchange performance, might well be adapted to the study of international systems. For not only are alliances Poisson-distributed, so are wars.[84]

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[83] This research is under way. In a recent paper we correlated our alliance flexibility scores with the Singer and Small interstate war data for the same period of time and for our five actors only. We found strong and statistically significant evidence for the hypotheses that alliance formation (hence balance of power system flexibility) is negatively associated with the occurrence of war and war magnitude, severity and intensity (R. M. Rood and P. J. McGowan, "Flexibility in Balance of Power Alliance Systems and International War," a paper delivered at the Third Annual Conference of the Southern Section of the Peace Science Society [International], Durham: Duke University, April 4–5, 1974). Our findings represent an independent replication of the well-known results of Singer and Small that alliance aggregation in the nineteenth century was negatively related to warfare; see Singer and Small, "National Alliance Commitments and War Involvement, 1815–1945," and "Alliance Aggregation and the Onset of War, 1815–1945."

[84] Richardson, *Statistics of Deadly Quarrels*, pp. 128–142; Singer and Small, *The Wages of War*, pp. 205–206.

# Models, Measurement and Sources of Error: Civil Conflict in Black Africa*

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Comparative politics has experienced both substantive and methodological progress during the past decade, but that progress has been hindered by inadequate communication between those with substantive or theoretical concerns and those with more technical interests. The literature on comparative analysis has warned of some fundamental problems which center on the notion of error and the distortions in results that error can produce. Most comparativists, however, fail to grapple with these problems. Too frequently researchers warn of possible bias but proceed as if results constitute verified knowledge. Comparativists concerned with improvement of theory or substantive knowledge must examine carefully these potential sources of error and estimate their effects on findings.

One area in political science that has experienced theoretical achievements as well as technical sophistication is the study of civil conflict, which became a major topic in cross-national research following Harry Eckstein's[1] plea for a consideration of general preconditions of strife rather than historical or particularistic explanations. Early empirical works focused on the dimensionality of conflict[2] and the testing of hypotheses relating various forms of conflict to conditions such as economic growth, land inequality, and inferred psychological states of individuals.[3] A body of theoretical literature offered conjectures on the causes of conflict,[4] while later quantitative works moved beyond hypothesis-testing research to the development of simple causal models which reflected on the proffered theories.[5] Over time these models have evidenced greater substantive detail and have been evaluated with increasingly sophisticated techniques. For example, two recent but independent studies, one by Hibbs[6] and one by Gurr and Duvall,[7] have presented complex models of civil conflict employing systems of simultaneous equations to approximate more adequately the complexity of full theory.

Most of these contributions to our knowledge of conflict involve large-scale cross-national analyses of aggregate data and are hence subject to several acknowledged but rarely assessed sources of error inherent in comparative analysis. The thrust of the research reported below is to investigate the impact of three fundamental problems of

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* I want to acknowledge the assistance of Raymond Duvall at many stages of this research effort, especially in the final revisions for the section on measurement error. Comments of unidentified referees also have been useful.

[1] Harry Eckstein, "On the Etiology of Internal Wars," *History and Theory*, 4 (1965), 133–163.

[2] See, for example, R. J. Rummel, "Dimensions of Conflict Behavior Within Nations, 1946–59," *Journal of Conflict Resolution*, 10 (March, 1966), 41–64; R. J. Rummel, "A Field Theory of Social Action with Application to Conflict Within Nations," *General Systems Yearbook*, 10 (1965), 183–211; R. J. Rummel, "Dimensions of Conflict Behavior Within and Between Nations," *General Systems Yearbook*, 8 (1963), 1–50; Raymond Tanter, "Dimensions of Conflict Behavior Within and Between Nations, 1958–60," *Journal of Conflict Resolution*, 10 (March, 1966), 41–64; Raymond Tanter, "Dimensions of Conflict Behavior Within Nations, 1955–60: Turmoil and Internal War," *Peace Research Society Papers*, 3 (1965), 159–183. For a summary of the dimensionality studies see Leo Hazelwood, "Concept and Measurement Stability in the Study of Conflict Behavior Within Nations," *Comparative Political Studies*, 6 (July, 1973), 171–195.

[3] See, for example, Ivo K. Feierabend and Rosalind L. Feierabend, "Aggressive Behaviors Within Polities, 1948–62: A Cross-National Study," *Journal of Conflict Resolution*, 10 (September, 1966), 249–271; Bruce Russett, "Inequality and Instability: The Relation of Land Tenure to Politics," *World Politics*, 16 (April, 1964), 442-454.

[4] See, for example, James Davies, "Toward a Theory of Revolution," *American Sociological Review*, 27 (February, 1962), 5–18; Johan Galtung, "A Structural Theory of Aggression," *Journal of Peace Research*, 1 (1964), 95–119; Ted Robert Gurr, "Psychological Factors in Civil Violence," *World Politics*, 20 (January, 1968), 245–278; Chalmers Johnson, *Revolution and the Social System*, Hoover Institution Studies #3 (Stanford: The Hoover Institution on War, Revolution, and Peace, Stanford University, 1964); Mancur Olson, "Rapid Growth as a Destabilizing Force," *Journal of Economic History*, 23 (December, 1963), 529–552.

[5] See, for example, Douglas Bwy, "Political Instability in Latin America: The Cross-Cultural Test of a Causal Model," *Latin American Research Review*, 3 (Spring, 1968), 17–66; T. R. Gurr, A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review*, 62 (December, 1968), 1104–1124; Manus Midlarsky and Raymond Tanter, "Toward a Theory of Political Instability in Latin America," *Journal of Peace Research*, 4 (1967), 209–227; Donald Morrison and H. M. Stevenson, "Integration and Instability: Patterns of African Political Development," *American Political Science Review*, 66 (September, 1972), 902–927.

[6] Douglas Hibbs, *Mass Political Violence* (New York: John Wiley, 1973).

[7] T. R. Gurr and Raymond Duvall, "Civil Conflict in the 1960's: A Reciprocal Theoretical System with Parameter Estimates," *Comparative Political Studies*, 6 (July, 1973), 135–169.

comparative analysis on one recent model of civil conflict. The three problems or sources of error are: measurement error (differences in score for the same concept for the same observation), sampling error (biases due to an unrepresentative sample), and error in theoretical specification (in particular, error caused by the exclusion of important theoretical variables). The model to be investigated is that developed by Gurr and Duvall as a formalization of Gurr's earlier theoretical work.[8] The concepts in that model will be measured by two distinct data sets to estimate measurement error; measures will be applied to a sample of black African nations, most of which were excluded from Gurr and Duvall's analysis, to partially assess sampling bias; and new concepts will be introduced to assess error in theoretical specification.

While the primary objective is to evaluate a major empirical effort at general theorizing on strife, the results of this research have important substantive implications for the Africanist as well as the general comparativist. Some argue, on the one hand, that data on African countries are so unreliable that quantitative studies of these nations are meaningless. If our evaluations indicate that the two African data sets provide very disparate results, that position is supported, but if results are comparable across data sets for the African sample, that position is weakened. A second argument states that efforts at generalization are useless since circumstances in Africa are so different from those elsewhere. It is believed that regional (or even country-specific) characteristics require explanations within the bounds of a particular region or type of system and that no general statements can hold across all systems. If the following analyses of the African sample correspond to Gurr and Duvall's findings, the generalist's position is strengthened, but if different relationships from the general model obtain in the African sample across both data sets, the country or area specialist's position is supported. Following a specification of the sample, the two data sets, and the measurement of concepts in each data set, each of the three sources of error will be discussed and their extent and effects estimated.

## Procedures for Model Evaluation

Gurr and Duvall present "an eleven variable, bloc recursive, simultaneous equation model."[9] Their eleven concepts are strain (structural constraints on the equitable distribution of values within and among societies), economic development (ECDEV), governmental centrism (CENT), political democracy (DEMOC), immediate past conflict (past MPC), historic traditions of conflict (TRAD), dissident institutional support (DIS), regime institutional support (RIS), foreign or external intervention (EXINT), stress (shortages or declines in supply of valued goods), and magnitude of political conflict (both turmoil and rebellion). In addition a multiplicative identity, tension, is incorporated in their model. They maintain that the variables can be divided into four subgroups:

(1) Strain, ECDEV, CENT, and DEMOC are as a group independent of other variables in the model. They are exogenous—that is, none of their variance is to be explained within the model. We can think of them as the more remote or "necessary" conditions of political conflict. Note too that they are relatively invariant over time. Few societies can rapidly reduce strain or rapidly change their gross level of economic productivity; nor do societies rapidly institutionalize either polyarchy or autocracy.
(2) Past MPC is a lagged endogenous variable.
(3) TRAD, DIS, RIS are an interrelated set of variables which partly determine one another. The expansion of dissident institutional support, for example, tends to occur at the expense of RIS, and vice versa. These three conditions are partly, not wholly, determined by the first two blocs of variables. They are also, we assume, more susceptible to change.
(4) The final bloc of variables includes stress, EXINT, and magnitude of political conflict (as well as the multiplicative term because of its stress component). They are closely linked to one another and, except EXINT, are largely determined within the model. All except the multiplicative term are "volatile," subject to considerable fluctuations over time.[10]

The primary interest here is in conflict, and therefore the original model is simply reduced to two blocs: (1) exogenous or pre-determined variables —strain, economic development, dissident institutional support, regime institutional support, past conflict (both turmoil and rebellion), and conflict traditions as a simple function of past conflict; and (2) endogenous variables—stress, external intervention, and conflict (both turmoil and rebellion), corresponding directly to the conflict bloc in the Gurr and Duvall model. Such modifications do not permit evaluation of the complete Gurr and Duvall model. In particular, I am unable to deal with the set of equations that they give as their first bloc in which regime institutional support, dissident institutional support, and conflict traditions are reciprocally endogenous. On the other hand, this modification permits an elimination of variables that do not appear in the conflict bloc, namely democracy and centrism, which in the African sample do not vary (at least as Gurr has judgmentally coded them), and it enables the reduction in importance of the conflict traditions concept, which in Gurr and Duvall's whole model plays a central role but which is relatively marginal to the conflict bloc. Changes in scoring of that concept that I have introduced (see below) should not, therefore, drastically affect results of analysis.

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[8] See, in particular, Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970). The Gurr and Duvall model has been chosen for this analysis since it is more fully specified and operationalized than others. Although Hibbs's work may be better known, it does not represent a single, fully developed body of theory.

[9] Gurr and Duvall, p. 135.

[10] *Ibid.*, pp. 141–142.

Gurr and Duvall assess their model of civil conflict with a sample of 86 nations for the period 1961–1965. The authors exclude from their sample a number of less technologically developed nations primarily because of missing or inadequate data, but as a result, their sample overrepresents Western, industrialized nations and raises the possibility of sampling bias. Their results, in other words, might not be general but might instead apply only to a restricted universe of relatively contemporary, "more developed" countries. They use primarily data that Gurr has reported elsewhere, although they do collect additional measures to supplement the earlier set. Gurr claims that his coders did extensive practice coding, but no formal reliability tests were conducted. Thus, a second problem with their work is the unknown magnitude of measurement error and, more important, the unestimated effects of error on the authors' results.

In an effort to assess the extent and impact of these possible sampling and measurement biases, I have replicated the Gurr and Duvall model employing two distinct data sets in a different sample that consists of 25 black African nations, all but seven of which were excluded from the Gurr and Duvall study.[11] This procedure provides only an imperfect test of sampling bias since numerous nations remain systematically excluded from both the 86-nation sample and the African sample. Most apparent is the absence of a truly spatially and temporally representative sample of societies. The African sample is no more a general representative sample than is Gurr and Duvall's. If the model proves invalid for the African context, however, one can conclude safely that it fails a weak test for sampling error and is probably far from general.

The first data set used in this study consists primarily of Gurr's for the black African countries. Because his data on these countries are not as extensive as for Gurr and Duvall's 86 nations, use is made of measures from the *World Handbook of Political and Social Indicators*.[12] The second data set consists of measures taken from Morrison *et al.'s Black Africa: A Comparative Handbook*[13] and supplementary *World Handbook* measures are employed for two indicators. Only one *World Handbook* indicator (government sanctions) is incorporated in both data sets; hence there is a minimum of shared data between them.

Gurr and Duvall follow certain procedures to measure their concepts, and the primary criterion in scoring concepts from these African data sets will be to follow as closely as possible the logic of their measurement procedures. In particular, they utilize two measurement strategies: (1) indirect measurement through multiple indicators, and (2) direct operationism. The more abstract concepts in their model, such as strain and stress, are indirectly measured. For these concepts, what is attempted here is to develop a set of seemingly valid indicators, subject them to the same validating procedures, and generate composite indices in a similar fashion. In this way one is probing measurement error as the differences in observational bases (particularly as manifested in different indicators) since the logic of the measurement procedures is comparable. When scoring more concrete concepts, Gurr and Duvall employ direct operationism in two forms. First, they use a single indicator for economic development, and I employ that same indicator as it is reported in two different sources. Measurement error in this case is attributable solely to differences in observational base. Second, for conflict and external intervention they assess the number of events and relevant attributes of events (e.g. number of participants). Here I use two different measurement procedures since one data set follows procedures parallel to Gurr and Duvall, while the other uses a more standard procedure of simple counting of events unweighted by attributes. In these cases, then, the assessment of measurement error involves not only differences in observational base but also differences in procedure.

Table 1 lists all of the concepts to be included in replications of the model and compares the indicators employed from each data set to measure each concept.[14]

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[11] The twenty-five countries are: Burundi, Cameroun, CAR, Chad, Dahomey, Ghana, Guinea, Ivory Coast, Kenya, Liberia, Malawi, Mali, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Somali, Sudan, Tanzania, Togo, Uganda, Upper Volta, Zaire, Zambia.

[12] Charles L. Taylor and Michael Hudson, *World Handbook of Political and Social Indicators*, 2nd ed. (New Haven: Yale University Press, 1972).

[13] Donald G. Morrison, Robert Mitchell, John N. Paden, and Hugh M. Stevenson, *Black Africa: A Comparative Handbook* (New York: Free Press, 1972).

[14] Concept scores are created by calculating the mean standard scores of all indicators of a concept. When a concept consists of several dimensions (e.g., economic, political, and social dimensions of stress), a mean standard score for each dimension is first cal-

**Table 1. Comparison of Concept Measurement Bases for two Data Sets and the Initial Model**

| Concept | Gurr and Duvall 86-Nation Study | Gurr Indicators for Present Sample | *Black Africa* Indicators for Present Sample |
|---|---|---|---|
| Strain | Economic Discrimination<br>Political Discrimination<br>Religious Cleavages<br>Separatist Feelings<br>Economic Dependence: negative net factor payments abroad as % GDP<br>Proportion exports to largest country | Economic Discrimination<br>Political Discrimination<br>Religious Cleavages<br>Separatist Feelings<br>Export Concentration by Commodity (W.H.) | Ethnic Unrepresentativeness of cabinet at Independence<br>Number of Languages<br>Language Classification<br>Ethnic Diversity: Hierarchy[a]<br>Ethnic Diversity: Stratification[a]<br>Ethnic Diversity: Agricultural Dependence[a]<br>Export Dependence<br>Trade as % GNP<br>% US Aid to Africa 1961 (sqrt)<br>% US Aid to Africa 1964 (sqrt)<br>Aid from East Bloc nations<br>Aid from Metropole 1963 |
| Economic Development | GNP per capita c. 1965 | GNP per capita c. 1965 (W.H.) | GNP per capita c. 1963 |
| Past Strife:<br>Turmoil and Rebellion | Regress Gurr 1961–65 on *World Handbook* data and retrodict for 1956–60 | Regress Gurr 1961–65 on *World Handbook* data and retrodict for 1956–60 | Regress *Black Africa* data 1961–65 on *World Handbook* data and retrodict for 1956–60 |
| Regime<br>Institutional Support | Party System Stability weighted by # parties<br>National Integration and centralization<br>Electoral Support<br>Union Support<br>Proportional Police Manpower | Growth in Central Gov't Expenditure<br>Size of Central Gov't Expenditure<br>Electoral Support<br>Union Support<br>Coercive Potential<br>Defense Expenditure per capita (W.H.) | Size Gov't Budget 1961<br>Expenditure/GNP 1961<br>Expenditure per capita 1966<br>Expenditure/GNP 1963<br>Legislative Support[b]<br>Defense Expenditure 1965<br>Defense Expenditure/GNP 1963<br>Military/population 1967<br>Change Police Size 1964–67 |
| Dissident<br>Institutional Support | Size, Organization and physical isolation of dissidents<br>Transportation Networks relative to area and population density<br>Size of Communist Parties weighted by party status<br>Size of Unions weighted by oppositional status | Size, Organization and physical isolation of dissidents<br>Transportation Networks relative to terrain | No comparable indicators—Gurr's DIS measure used |
| Conflict Traditions | Durability of Regime and Scope and Success of Internal War 1850–1960<br>Frequency and Success of Coups 1900–60<br>Recency of Last Military Intervention, 1960<br>Union Opposition Status, 1955–65 | Past Turmoil + Past Rebellion | Past Turmoil + Past Rebellion |
| Stress | Short Term Trend in export Value 1950–60<br>Declines in Short Term Trends in Export Value, 1961–65<br>Defense Expenditure as % GNP<br># of Negative Gov't Sanctions<br>Proportional Military Manpower (Europe)<br>Proportional Police Manpower (Europe) | Trade Declines X Economically Mobilized Population<br>Growth Energy Consumption, reversed (W.H.)<br>Negative Government Sanctions (W.H.)<br>Downturns in Secondary Education | Decline GDP<br>Decline GNPCAP<br>Decline Energy<br>Decline Primary School Enrollments, 1960–66<br>Decline Secondary School Enrollments, 1962–66<br>Decline Literacy, 1950–65<br>Negative Government Sanctions (W.H.) |

[a] Scores indicate diversity among ethnic groups in a country.

[b] To create an indicator parallel to Gurr's electoral support, the author coded each country on the average percentage of legislative seats held by the ruling party 1961–65.

**Table 1. (Continued)**

| Concept | Gurr and Duvall 86-Nation Study | Gurr Indicators for Present Sample | *Black Africa* Indicators for Present Sample |
|---|---|---|---|
| External Intervention | Extent of External Support for Dissidents | Extent of External Support for Dissidents | Number of External Interventions (W.H.) |
| Conflict: Turmoil | Extent and Intensity of Riots, Demonstrations, Strikes, Clashes, Local Uprisings | Extent and Intensity of Riots, Demonstrations, Strikes, Clashes, Local Uprisings | Number of riots, Demonstrations Strikes, 1961–65 |
| Rebellion | Extent and Intensity of Plots, Coups, Terrorism, Civil Wars, etc. | Extent and Intensity Plots, Coups, Terrorism, Civil Wars, etc. | Number of Civil Wars, Rebellions, Irredentism, Feuds, Revolts, 1961–1965 |
| Tension | (Stress+Strain)×Traditions | (Stress+Strain)×Traditions | (Stress+Strain)×Traditions |

In general, as indicated above, Gurr's data on the African countries can be seen to be comparable to, though less extensive than, indicators for the 86 countries. *Black Africa* data consist of somewhat different variables but provide numerous indicators to tap the major concepts.

For particular concepts, Table 1 reveals the following:

(1) The only concept for which neither *Black Africa* nor *World Handbook* measures could be found was dissident institutional support, and hence two of Gurr's measures are employed for both analyses.

(2) In their concept of strain Gurr and Duvall include measures of four dimensions: economic discrimination, political discrimination, social cleavages, and international economic dependence (export concentration). *Black Africa* data supply indicators of only three of these dimensions: social diversity or cleavages, economic dependence (export, trade, aid), and political discrimination (ethnic unrepresentativeness of cabinet).[15]

(3) Comparable measures are available for economic development, since all data sets have information on GNP per capita, the sole indicator employed by Gurr and Duvall.

(4) Gurr and Duvall measure past strife by regressing their turmoil and rebellion scores for 1961–65 on the *World Handbook's* four indicators of strife (number of deaths, riots, demonstrations, and armed attacks) for the same period. They then estimate past strife by applying the regression equation to *World Handbook* data for 1955–60. The same procedure is employed here for the African sample, regressing Gurr measures of conflict in the one case, and *Black Africa* measures in the other.

(5) As expected, the concept of regime institutional support, at least as Gurr and Duvall measure it, involves the dimensions of size, support, and coercive potential of the national government. While exact indicators differ from the Gurr and Duvall study, all three of these dimensions seem to be tapped adequately by both Gurr and *Black Africa* data.

(6) The indicators that Gurr and Duvall propose for traditions of conflict simply are not appropriate for the African sample. Regime durability as of 1960, and frequency and recency of

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culated, and then the mean of the dimensions obtained. Gurr and Duvall use a similar simple additive model for creating RIS, DIS, and TRAD scores. In scoring strain and stress, however, they regress the separate indicators on their measures of conflict and then weight each indicator according to the structure of the regression equation. That procedure was not employed here since it results in several scores for the same concept and also since it seems a bit arbitrary.

[15] Since Gurr and Duvall include political discrimination in their measure of strain it is included in the following analyses. Evidence suggests, however, that political discrimination has different effects than do the other components of strain. Regressions of the strain components on conflict for both data sets reveal that other forms of discrimination promote conflict, while political discrimination tends to reduce conflict. Consider, for example, turmoil:

Gurr turmoil = .36 export concentration + .39 economic discrimination + .20 religious cleavages + .11 separatist feelings − .03 political discrimination − 2.16469

*Black Africa* turmoil = .34 economic discrimination + .44 social differences + .50 external dependence − .48 political discrimination + 5.04637

Gurr and Duvall also report in a footnote that political discrimination seems to reduce conflict, unlike the other components of strain, but they proceed to include it in their concept score. Thus, they give positive weights to the other strain indicators and a negative weight to political discrimination. This procedure seems to deviate from their conceptualization of strain, which is viewed as a positive contributor of conflict. Since the negative impact of political discrimination is most marked in the *Black Africa* data, two measures of strain are created and employed in later analyses, one including and one excluding political discrimination.

coups prior to 1960 are generally irrelevant to African countries, most of which were still under colonial rule until 1960. The closest approximation to conflict traditions is immediate past conflict, which is distinguished by Gurr and Duvall as a separate concept. I have collapsed that distinction, however, and scored traditions as the sum of past conflict scores (turmoil and rebellion) for each data set.

(7) Stress measures for Gurr and Duvall involve short-run economic declines and political restrictions. Both data sets provide indicators of economic downturns, and both provide measures of educational declines to tap an additional dimension of social stress. The *World Handbook* variable of government sanctions is used to tap political restrictions for both analyses.

(8) For external intervention, Gurr and Duvall employ a coded variable which estimates the extent of external support for dissidents, and the same measure is available for the African sample. Since *Black Africa* does not have a comparable indicator, the number of external interventions listed in the *World Handbook* is employed for the second data set.

(9) The final and central concept to be measured is political conflict. Gurr's measures of conflict for the African sample include the extent and intensity of turmoil (events with limited political objectives) and rebellion (efforts to bring about more fundamental change in the personnel or structure of government). Extent is measured by summing the man-days of participation which are estimated by multiplying the number of nongovernmental participants by the event's duration in days. Intensity is measured by summing the deaths associated with all conflict events. Gurr's measures of turmoil and rebellion used here for the African countries are identical to measures used by Gurr and Duvall in the 86-nation study. In contrast, *Black Africa* measures of conflict consist solely of the number of events summed, with no weightings for duration, intensity, scope of participation, or population size.[16]

### Measurement Error

The first source of bias in the statistical evaluation of general models is measurement error, the problem of inaccuracy in concept scores. In attempting to assess such error, and its impact on inferences drawn from the analysis of data, one is concerned with the twin problems of validity and reliability. Both of these issues are closely related to, and often confused with, problems of inference imposed by sampling biases. In this section and the next, I attempt to disentangle the two primary sources of error and to estimate the extent and nature of each.

Validity refers to the degree to which we are measuring what we think we are measuring. Gurr and Duvall assess validity through face validation (an argument which logically justifies an indicator as an adequate representation of a concept), and also through construct validation, a procedure in which one judges the validity of a concept by the degree to which the concept relates to other concepts to which it is theoretically related. Gurr and Duvall utilize conflict as the construct according to which measuring procedures for other concepts are validated and discard as invalid indicants which do not correlate adequately with conflict. In the present study, I have attempted to replicate their validation procedures. Although the primary criterion in developing indicators from the two data sets was to approximate Gurr and Duvall's measures as closely as possible, every seemingly valid indicator of each concept was correlated with measures of conflict and any indicator that correlated strongly in a direction other than that predicted by theory was not included.

The issue of validity becomes quite complicated in cross-national research, for it is related to the problem of conceptual equivalence. It is possible, indeed probable, that indicators which validly represent a concept in one cultural context have different meanings, and hence may not reflect the same concept, in another cultural setting. This problem implies that indicators developed by Gurr and Duvall might not be valid indicators for the African sample. The criticism would not be directed at the validity of their measures for their 86 nations but rather at the validity of their measures when applied to African countries in this particular research. Later efforts to weed out various sources of error necessitate fairly exact replication of the general model, but by adopting nearly identical indicators, I may be introducing invalid measurements for the African sample. Although some indicators in the African sample do differ somewhat from the general model and although construct validation is employed, the primary criterion in concept measurement is comparability. There exists, then, a constant struggle between efforts to obtain cross-research comparability and cross-cultural equivalence. The problems of validity and sampling bias now can be seen to be closely interwoven, since attempts to extend the sample may threaten validity, and invariant measurement rules may apply validly to a very restricted sample.

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[16] Turmoil data have been collected by the authors of *Black Africa* but are not published in that book because of questionable reliability. Turmoil data begin with the year of independence. Pre-independence years for countries independent after 1960 have been updated with *World Handbook* data. Taylor and Hudson code days of rioting, demonstrating, and so forth, rather than the number of events of each type. For comparability, five days were considered roughly equivalent to one event.

Beyond the initial face validation of separate indicators (dependent on the judgment of Gurr and Duvall), and the simple construct validation of correlating indicators with conflict, the only method to assess measurement validity for the African sample has been a second-order face validation. That is, does one "feel" that the distribution of concept scores among the African countries adequately reflects the nature of the concept? Does one agree that countries ranking high on strain, for example, in reality experience more strain than countries ranking lower? While readers must draw their own conclusions,[17] two general points should be made here. First, on grounds of previous and cumulative knowledge, one can challenge the validity of some of the measurement procedures. GNP per capita is probably not a sufficient indicator of the concept economic development whose valid measurement would require tapping far more than gross productivity. Similarly the validity of Gurr's external intervention is probably weakened by relying solely on support for dissidents and not including interventions for established governments. These issues, however, are not my major concern here. Second, validity is never a directly assessable concept, so that what has been done here is virtually the extent practicable. For that reason, I do not pursue the issue of validity any further in what follows.

Reliability is a problem of the consistency and accuracy of our measures, assuming their validity. A considerable body of literature spells out sources of observational error, methods of controlling such error, and techniques to limit the biases that unreliable data can produce in cross-national or cross-cultural research.[18] But since the data and measurement techniques for this research are largely given, the more appropriate question relates to estimating the extent of error already existing in the data. However, in assessing the impact of unreliable measurement procedures, one again becomes involved in the related problem of sampling bias, for we have reason to believe that data on African countries are less accurate than for most other nations. In attempting to determine sources of error in aggregate data, Rummel has found that the level of economic development is related to some measures of error.[19] African nations, being among the least technologically developed, would be expected to yield relatively more erroneous data. In other words, indicators *could* be relatively reliable measures for the 86-nation study, but considerably less reliable for the African sample. Estimates of measurement error, then, reflect only indirectly on Gurr and Duvall's work.

Standard tests of reliability are not applicable for assessing error in the African data. Interitem correlation is inappropriate since some concepts are directly and singularly operationalized and since most other concepts consist of several conceptual dimensions (e.g. strain). In addition, Gurr's failure to report intercoder agreement, and the coding of *Black Africa* data by single coders mean that standard intercoder tests of reliability are impossible. I offer, however, a test that is similar in logic to intercoder agreement, in being based on two applications of equivalent instruments to the same observational set. That is, if for the same countries, measures of a concept from two independently collected data sets interrelate highly, then the measures can be assumed to be reliable (or systematically unreliable); if there is little relationship between two concept scores, then measurement is not reliable. Low correlations unambiguously reflect low reliability, and high correlations are only suggestive of high reliability, since they can also reflect systematic inaccuracy.

Consider, for example, the following model of measurement:

$$X' = X + f(X) + g(Z) + c + e,$$

where: $X'$ = observed score
$X$ = true score
$Z$ = other variables
$c$ = some constant
$e$ = random disturbance.[20]

The nature of correlations is such that the linear components of the measurement model have no effect on correlation coefficients. Therefore, if the correlation between two measures of the same concept remains close to 1.00 (i.e. seemingly reflects high reliability) observed scores can contain appreciable error but that error must be systematic and linear in at least one of the observed scores. Alternative interpretations for high correlations between scores is small and random error or nonlinear systematic error that is of the same form in both sets. On the other hand, when the correlations differ greatly from 1.00 it can be concluded safely that at least one of the scores contains a large random component and/or a nonlinear systematic component (i.e., $f(X)$ and/or $g(Z)$ are nonlinear functions).

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[17] Concept scores and rank orders on each concept for the twenty-five nations are available from the author.

[18] See, for example, Donald T. Campbell, "Techniques for Determining Cultural Biases in Comparative Research," in *Comparative Perspectives: Theories and Methods,* ed. Amitai Etzioni and Frederick L. Dubow (Boston: Little, Brown, 1970), pp. 407–410; Kenneth Janda, "Data Quality Control and Library Research on Political Parties," in *A Handbook of Method in Cultural Anthropology,* ed. Raoul Naroll and Ronald Cohen (Garden City: National History Press, 1970), pp. 962–973; Raoul Naroll, "Data Quality Control in Cross-Cultural Surveys," in Naroll and Cohen, pp. 927–945; R. J. Rummel, "Dimensions of Error in Cross-National Data," in Naroll and Cohen, pp. 946–961; Raoul Naroll, *Data Quality Control—A New Research Technique* (New York: Free Press, 1962); Bruce Russett, "Techniques for Controlling Error," in *Statistical and Quantitative Methods,* ed. Michael Haas (Evanston: Northwestern University Press, 1972).

[19] Rummel, "Dimensions."

[20] By random disturbance I mean that: $E(e_i) = 0$, all $i$; $E(e_i e_j) = 0$, all $i$, $j$, $i \neq j$; $E(e_i^2) = \sigma_e^2$, all $i$.

Thus one can draw some conclusions about the nature and extent of measurement error from correlations between two measures of a concept. Table 2 reports the proportion of unshared variance in concept scores (1.00 minus the squared correlations). The higher the unshared variance, the less reliable the score. One sees that past turmoil, economic development, and past rebellion share most of their variance in common across data sets. At this point all that can be concluded is that error is of one of three forms: (1) small random, (2) linear systematic, or (3) common, nonlinear systematic. For the other six concepts, error is extensive, and it is either random or systematic of a different nonlinear form in each of the data sets. While it is not surprising that the greatest unreliability is evidenced for the three concepts that involve different measurement procedures (turmoil, rebellion, external intervention), it is perhaps surprising that the three multiply indicated concepts are so unreliable (stress, strain, regime institutional support).

Table 2. Unshared Variance for Concepts Across Data Sources[a]

| Concept | Unshared Variance |
|---|---|
| External Intervention | .73 |
| Rebellion | .65 |
| Turmoil | .59 |
| Stress | .54 |
| Strain | .54 (.44)[b] |
| Regime Institutional Support | .48 |
| Past Rebellion | .21 |
| Economic Development | .17 |
| Past Turmoil | .10 |

[a] Dissident institutional support is excluded from the table since the same score is used for both data sets. Tension also is excluded since it is based on other concept scores.

[b] The first strain figure is the unshared variance between the Gurr score and the *Black Africa* score which replicates the Gurr score; the second strain figure is the unshared variance between the Gurr score and the *Black Africa* score, which excludes political discrimination. See footnote 15.

It is important to assess how much of the unreliability in these six concepts is due to nonlinear systematic error. One can probe further the nature of error by correlating these concepts with dissident institutional support which is the same in both data sets. Table 3 reports the relevant correlations. If two scores for the same concept correlate very differently with a single referent (dissident institutional support), then systematic error is involved in at least one of the two scores. Random error attenuates the correlations but the attenuation is expected to be similar in each case. It is apparent in Table 3 that dissident institutional support correlates differently with each of the two measures for turmoil, rebellion, strain and perhaps stress, pointing to significant nonlinear systematic error in at least one of the scores for each concept. On the other hand, the similar correlations involving external intervention and regime institutional support suggest the predominance of random error.

Information in Tables 2 and 3 helps to estimate the extent and nature of error in concept scores. A final point of interest for later attempts to estimate the impact of error is the extent to which systematic error is shared across concepts within a data set. The basis for these inferences rests on a comparison of correlations among concepts across data sets. Table 4A includes the correlations among the four variables concluded to contain extensive nonlinear systematic error. Interpretation of this table is based on the following logic: If the correlations between two concepts known to contain extensive systematic error differ markedly across data sets, the form of the systematic error is not similar across concepts in at least one data set; if the correlations for concepts known to be systematically erroneous are similar and also large, then systematic error is of the same form across concepts within data sets. The application of this logic to Table 4A leads to the conclusion that turmoil, rebellion, and stress share systematic error in both data sets and that the systematic error is of a different form in each of the data sets. The systematic error in strain, on the other hand, is not of the same form as that for turmoil, rebellion, and stress in at least one of the data sets.

The final question in the assessment of measurement error is the determination of the likelihood of systematic error in seemingly reliable variables (economic development, past turmoil, past rebellion). Table 4B reports the correlations between these variables and those containing extensive, shared systematic error (turmoil, rebellion, stress). In interpreting this table the following logic is used: If correlations of seemingly reliable scores with systematically erroneous scores are

**Table 3. Correlations with Dissident Institutional Support for Both Data Sets**

| Variable | *r*<br>Gurr | *r*<br>*Black Africa* | Difference in Covariance | Inferred Error[a] |
|---|---|---|---|---|
| Turmoil | .27 | .50 | .18 | *S* |
| Rebellion | .56 | .61 | .06 | *S* |
| Strain | .33 | .22 | .06 | *S* |
| Stress | .49 | .45 | .04 | ? |
| External Intervention | .36 | .32 | .03 | *R* |
| Regime Institutional Support | −.41 | −.43 | .01 | *R* |
| Economic Development | −.27 | −.32 | .03 | *R* |
| Past Turmoil | .44 | .46 | .02 | *R* |
| Past Rebellion | .39 | .42 | .02 | *R* |

[a] The determination of the size of the difference in covariance needed to distinguish systematic from random error is based on the correlations involving economic development, past turmoil, and past rebellion, which are the variables inferred in Table 2 to contain little or no nonlinear systematic error of appreciable difference across data sets. *S* stands for systematic error, and *R* stands for Random error.

similar across data sets, then the former are not sharing appreciable nonlinear systematic error with the latter in either set;[21] if, on the other hand, correlations are different across data sets, then the form of systematic error might be shared with turmoil, rebellion, and stress in at most one data set. Where the correlations are high, this possibility assumes a certain likelihood. From this logic, one can conclude that economic development is not systematically erroneous in a form similar to turmoil, rebellion, and stress in either set but that past turmoil and past rebellion probably share systematic error particularly with turmoil and rebellion in one of the data sets.

**Table 4. Comparison of Correlations Among Variables Between two Data Sets**

| Variables | *r*<br>Gurr | *r*<br>*Black Africa* | Difference in Covariance | Shared Systematic Error[a] |
|---|---|---|---|---|
| A. Variables With Systematic Error | | | | |
| Turmoil with Stress | .43 | .50 | .07 | yes |
| Turmoil with Rebellion | .27 | .45 | .13 | yes |
| Rebellion with Stress | .51 | .64 | .15 | yes |
| Strain with Rebellion | .26 | .18 | .04 | no |
| Strain with Turmoil | .51 | .12 | .25 | no |
| Strain with Stress | .07 | .06 | .00 | no |
| B. Seemingly Reliable Variables With Systematically Erroneous Variables | | | | |
| Economic Development with Turmoil | −.08 | .01 | .01 | no |
| Economic Development with Rebellion | −.21 | −.07 | .03 | no |
| Economic Development with Stress | .11 | −.25 | .07 | no |
| Past Turmoil with Turmoil | .42 | .65 | .24 | yes |
| Past Turmoil with Rebellion | .35 | .67 | .33 | yes |
| Past Turmoil with Stress | .59 | .71 | .15 | ? |
| Past Rebellion with Turmoil | .03 | .40 | .16 | yes |
| Past Rebellion with Rebellion | .39 | .62 | .23 | yes |
| Past Rebellion with Stress | .49 | .56 | .07 | ? |

[a] Although no conventional guide exists, a difference in covariance of roughly .15 is used as a cutting point to distinguish similar from dissimilar correlations.

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[21] The reason for this logic is that the form of error for the systematically erroneous variables is known to be different for the two data sets and nonlinear in at least one of them, whereas the seemingly reliable variables can contain nonlinear systematic error only if the nonlinear form is similar across data sets. Therefore, shared systematic error is possible for a maximum of one data set and is compatible only with differences in correlations across data sets.

Three conclusions follow from this analysis of measurement error. First, we have some knowledge of the probable form and extent of error in the African data. In particular, (1) economic development has small and probably random error; (2) regime institutional support and external intervention contain large, random error; (3) strain has some systematic error; (4) stress, rebellion, and especially turmoil have large, shared systematic error; and (5) past turmoil and past rebellion contain some systematic error, which in one of the data sets is shared with turmoil, rebellion, and perhaps stress.

Second, although measurement error can be assessed directly only for the African data, the findings have direct implications for the Gurr and Duvall study. Because turmoil, rebellion, and stress are known to have shared systematic error in both data sets, and because one set of measures for turmoil and rebellion are identical to measures in the 86-nation study, the likelihood that turmoil, rebellion, and probably stress share systematic error in the Gurr and Duvall study is very high.

Third, measurement procedures popular in contemporary political science are implicated. Each of the five concepts believed to share systematic error involve the counting of numbers of events (stress contains other indicators as well). The uniformly higher correlations among these concepts for the *Black Africa* data (see Tables 4A and 4B) suggest that systematic error is of a more similar form across concepts when measurement is based on simple counting of events rather than on scoring attributes of these events. This is not to say that the *extent* of systematic error is greater, but rather that given a certain magnitude of error, the *form* is more similar under procedures of simple tabulation.

### Sampling Error

A second major source of error in comparative analysis results from the use of nonprobability samples. Large-scale cross-national studies often analyze all countries on which relevant data are available or else focus on regional subsamples. Neither alternative provides the analyst with a representative sample of the population of nation-states. Instead, the generalizability of results is always suspect because variables may behave differently in different subsamples.

Gurr and Duvall's 86-nation study clearly is subject to sampling error since the authors systematically underrepresent the poorer nations of the world because of data availability. We would like to know, then, if in fact variables behave in other subsamples as they do in Gurr and Duvall's 86-nation sample, so as to estimate if sampling error exists. One way to estimate such error is to compare correlations among concepts in the two African data sets with correlations in the Gurr and Duvall sample. When correlations in the two African samples are similar but both differ from the 86-nation study, we have evidence of sampling error, i.e., that relationships are different in the African subsample.

Table 5 provides the correlations among the concepts that appear to behave differently in the two subsamples—strain, economic development,

**Table 5. Comparison of Correlations Across Samples**

| Variables | Gurr Data African Sample | *Black Africa* Data African Sample | Gurr-Duvall Data 86-nation Sample |
|---|---|---|---|
| Strain with Past Turmoil | −.01 | .01 | .46 |
| Strain with Past Rebellion | .06 | .10 | .38 |
| Strain with Stress | .07 | .06 | .41 |
| Strain with External Intervention | −.05 | −.09 | .53 |
| Strain with Rebellion | .26 | .18 | .64 |
| Economic Development with Past Turmoil | −.04 | −.10 | −.34 |
| Economic Development with Past Rebellion | −.07 | −.08 | −.43 |
| Economic Development with External Intervention | −.07 | −.14 | −.37 |
| Economic Development with Turmoil | −.08 | .01 | −.36 |
| Economic Development with Rebellion | −.21 | −.07 | −.48 |
| Economic Development with Tension | −.01 | −.06 | −.61 |
| Economic Development with Stress[a] | .11 | −.25 | −.59 |
| Regime Institutional Support with Rebellion | −.23 | −.23 | −.52 |
| Regime Institutional Support with Tension | −.01 | −.26 | −.55 |
| Regime Institutional Support with Dissident Institutional Support | −.41 | −.43 | −.62 |
| Regime Institutional Support with Past Turmoil | −.16 | −.15 | −.34 |
| Regime Institutional Support with External Intervention[a] | .02 | −.18 | −.34 |

[a] Differences across samples are questionable because of the instability of correlations across the two African data sets.

and regime institutional support. While Gurr and Duvall find strong relationships between strain and a majority of their concepts, strain scores in Africa have little relationship to other model concepts. Either Gurr and Duvall's measures of strain are not valid measures of the concept in Africa, or else strain has different effects in the African context. Economic development relates strongly and negatively to most of Gurr and Duvall's concepts, but in Africa little relationship obtains. This finding probably indicates that the threshold at which economic development affects phenomena such as conflict has not been reached yet by most African nations and that among 25 poor nations economic development is unrelated to other model concepts. Similarly, regime institutional support demonstrates somewhat different patterns in Africa, primarily in that its effects are less strong than in the 86-nation sample.

While many correlations are similar across data sets and across samples we do have evidence that some variables intercorrelate differently across samples and hence that Gurr and Duvall's conflict model is subject not only to measurement error but also to sampling error. Two caveats are worth mention, each deriving from the previous discussion of measurement error. First, I probably *under*estimate sampling error to some extent because past turmoil and past rebellion are expected to have different correlations in the two African samples. Second, I probably *over*estimate sampling error because the large random components in regime institutional support and probably strain lead to the expectation of attenuated correlations in the African sample, at least if error is greater for the African countries than others. Overall, sampling error for Gurr and Duvall seems less extensive than measurement error for African countries.

## Estimating the Impact of Measurement and Sampling Error

The model that Gurr and Duvall offer is in structural equation form; regression techniques are used to estimate structural parameters.[22] The basic assumptions of regression analysis imply first, that there is no specification error, a problem discussed in the following section; second, that there is no systematic error in any variable(s); third, that random error can exist only in the dependent variable; and fourth, that there is no sampling error (variables on observations in the sample do not have a different probability distribution than do variables in the universe of observations). Violations of any of these assumptions can lead to biased estimates.[23]

The two previous sections indicated that our measures of concepts and the nature of the sample do in fact probably violate the basic assumptions of inferential regression analysis. There was evidence that dependent variables such as turmoil and rebellion contain systematic error in the African sample; that many of the independent variables are quite erroneous, often including some systematic component; and that sampling error is operative since some concepts appear to behave considerably differently in the two samples. The *potential* effects of such violations are severe, but it would be more interesting to discover what the *actual* effects are. It is an easy escape for the researcher to warn the reader that his results might be biased if in fact assumptions are violated, but our knowledge of political phenomena, conflict being the case in point, could develop more rapidly if we had estimates of *how* biased our findings actually are. Thus, the central concern of this research effort has been to go beyond the suggestion and discovery of errors to estimate their actual effects, both in the African sample and in the Gurr and Duvall general model. Analyses reported below enable an estimation of the impact of error in concept measurement for the black African countries, although they do not provide a basis for directly judging the accuracy of measurement in any larger sample. Results do point to concepts, however, that are likely to be most unreliably measured in non-African nations. In addition, the following analyses enable direct assessment of sampling error in the general model by determining if results based on a larger number of countries apply to the black African countries. Thus, conclusions will be directed to the effects of sampling error on the Gurr and Duvall general model and of measurement error by students of African politics.

The strategy followed to estimate the impact of errors is to treat sampling and measurement errors together. Gurr and Duvall's equations will be presented, and the presentation will include not only their parameter estimates but also confidence intervals for those estimates (the range in which the "true" value is most likely to fall). The equations are replicated in the black African sample using both data sets. Parameter estimates in these equations are essentially descriptive of the African sample so that the primary concern is to see if they fall within the confidence intervals of the param-

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[22] Gurr and Duvall ("Civil Conflict in the 1960's") provide a brief discussion of the techniques used, and Hibbs (*Mass Political Violence*) provides a helpful appendix on two-stage-least-squares regression. Readers unfamiliar with these techniques might find both discussions useful.

[23] These assumptions were derived from discussions in John Johnston, *Econometric Methods* (New York: McGraw Hill, 1963), p. 232, and Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley, 1964), pp. 299–300.

**Table 6. Equations for Estimating the Impact of Measurement and Sampling Error[a]**

A. STRESS

Determinants of Stress

| Sample and Data Set | Strain | EcDev | Turlag | | | $R^2$ |
|---|---|---|---|---|---|---|
| Gurr-Duvall<br>86 Nations<br>(Turmoil) | +.27<br>(+.45–+.09) | −.51<br>(−.69––.33) | +.29<br>(+.46–+.12) | | | .48 |
| Gurr Data<br>Africa Sample | +*.09* | +.14 | +.60 | | | .37 |
| Bl. Africa Data<br>Africa Sample | +*.14* | −.23 | +.68 | | | .55 |
| | Trad | EcDev | Reblag | Exint | | |
| Gurr-Duvall<br>86 Nations<br>(Rebellion) | +.15<br>(+.36––.06) | −.26<br>(−.46––.06) | +.34<br>(+.54–+.14) | +.23<br>(+.44–+.02) | | .45 |
| Gurr Data<br>Africa Sample | +.63 | +.15 | +.04 | −.03 | | .42 |
| Bl. Africa Data<br>Africa Sample | +.67 | −*.17* | −.04 | +*.08* | | .58 |
| | EcDev | Turlag | Rebell | R.I.S. | D.I.S. | |
| Best Equation<br>Gurr Data<br>Africa Sample | +.40 | +.38 | +.37 | −.27 | +.11 | .56 |
| Replication with Black Africa Data | −.01 | +.46 | +.40 | −.35 | −.17 | .65 |
| | | Turlag | Rebell | R.I.S. | | |
| Best Equation<br>Bl. Africa Data<br>Africa Sample | | +.54 | +.17 | −.33 | | .65 |
| Replication with Gurr Data | | +.45 | +.39 | −.04 | | .45 |

B. EXTERNAL INTERVENTION

Determinants of External Intervention

| Sample and Data Set | Rebell | Strain | D.I.S. | | | $R^2$ |
|---|---|---|---|---|---|---|
| Gurr-Duvall[b]<br>86 Nations | +.58<br>(+.83–+.33) | +.06<br>(+.33––.27) | +.03<br>(+.24––.18) | | | .45 |
| Gurr Data<br>Africa Sample | +.84<br>+1.30 | −.31 | −.26 | | | .56<br>.60 |
| Bl. Africa Data<br>Africa Sample | +*.71*<br>+1.02 | −*.22* | −.26 | | | .38<br>.42 |
| | Rebell | | D.I.S. | Tension | Turmoil | |
| Best Equation<br>Gurr Data<br>Africa Sample | +1.09 | | −.36 | +.40 | −.46 | .67 |
| Replication with Black Africa Data | +.59 | | −.23 | +.28 | +.10 | .46 |
| | Rebell | Strain | | Tension | | |
| Best Equation<br>Bl. Africa Data<br>Africa Sample | +.28 | −.21 | | +.50 | | .50 |
| Replication with Gurr Data | +.84 | −.31 | | +.17 | | .66 |

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[a] Italicized coefficients are those that fall within the .10 confidence limits of the Gurr-Duvall parameter estimates. Parameter estimates are given as standardized coefficients. Coefficients for the African sample are standardized over that sample rather than relative to the 86 nations. Equations were estimated as a simultaneous system with two-stage least squares techniques.

[b] The Gurr-Duvall model can be viewed either as a theoretical specification of three causal variables (rebellion, strain, dissident strength), or as an empirical conclusion of a single causal variable (rebellion).

[c] The second equation based on *Black Africa* data incorporates the Strain score which eliminates Political Discrimination. P.D. refers to the separate Political Discrimination score. See footnote 15.

**Table 6. (Continued)**

C. TURMOIL

| Sample and Data Set | Determinants of Turmoil<br>Strain | R.I.S. | EcDev | Trad | Tension | | $R^2$ |
|---|---|---|---|---|---|---|---|
| Gurr-Duvall<br>86 Nations | +.41<br>(+.71–+.11) | −.27<br>(−.47––.07) | +.15<br>(+.31––.01) | +.25<br>(+.51––.01) | +.36<br>(+.80––.08) | | .56 |
| Gurr Data<br>Africa Sample | +*.55* | −*.29* | +*.18* | +.89 | −.66 | | .35 |
| Bl. Africa Data<br>Africa Sample | +.01 | −*.41* | +*.31* | +*.45* | +*.14* | | .51 |
| | *Str.* +*.52*<br>*P.D.*[c] −.39 | −*.42* | +.42 | +1.44 | −1.32 | | .64 |
| | Strain | Exint | Stress | Turlag | Reblag | Rebell | |
| Best Equation<br>Gurr Data<br>Africa Sample | +.39 | −.49 | +.37 | −.48 | −.32 | +.34 | .60 |
| Replication with Black Africa Data | +.11 | −.04 | +.50 | +.46 | −.17 | −.02 | .39 |
| | *Str.* +.48<br>*P.D.*[c] −.29 | −.05 | +.15 | +.55 | +.05 | −.36 | .58 |
| | D.I.S. | R.I.S. | EcDev | Turlag | Reblag | | |
| Best Equation<br>Bl. Africa Data<br>Africa Sample | +.20 | −.56 | +.46 | +.79 | −.39 | | .66 |
| | *Str.* +.23<br>*P.D.*[c] −.22 | −.49 | +.42 | +.66 | −.27 | | .70 |
| Replication with Gurr Data | +.02 | −.65 | +.33 | +.55 | −.44 | | .44 |

D. REBELLION

| Sample and Data Set | Determinants of Rebellion<br>R.I.S. | Tension | Exint | | | $R^2$ |
|---|---|---|---|---|---|---|
| Gurr-Duvall<br>86 Nations<br>(Rebellion) | −.11<br>(−.25–+.03) | +.52<br>(+.81–+.23) | +.22<br>(+.50––.06) | | | .63 |
| Gurr Data<br>Africa Sample | −*.21* | +.11 | +.61 | | | .61 |
| Bl. Africa Data<br>Africa Sample | −*.02* | +.86 | +*.05* | | | .63 |
| | Strain | EcDev | D.I.S. | Tension | Exint | |
| Gurr-Duvall<br>86 Nations<br>(Deaths) | +.33<br>(+.58–+.08) | −.02<br>(−.15–+.11) | +.10<br>(+.27––.07) | +.47<br>(+.76–+.18) | +.15<br>(+.49––.19) | .72 |
| Gurr Data<br>Africa Sample | +*.19* | −*.08* | +.28 | .00 | +.53 | .69 |
| Bl. Africa Data<br>Africa Sample | +.03 | +*.06* | +.28 | +*.64* | +*.13* | .70 |
| | D.I.S. | Exint | Turlag | Turmoil | | |
| Best Equation<br>Gurr Data<br>Africa Sample | +.33 | +.69 | −.27 | +.36 | | .70 |
| Replication with Black Africa Data | +.42 | +.30 | +.42 | −.19 | | .64 |
| | D.I.S. | Tension | Exint | Turmoil | | |
| Best Equation<br>Bl. Africa Data<br>Africa Sample | +.32 | +.90 | +.07 | −.34 | | .69 |
| Replication with Gurr Data | +.36 | −.17 | +.61 | +.30 | | .68 |

eter estimates generated by Gurr and Duvall. In addition, best descriptive equations are generated for each African data set and replicated in the other to assess correspondence of results for the same sample across data sets. To sort out sampling and measurement distortions, the following criteria are used:

(1) If both of the African data sets generate equations similar to Gurr and Duvall's, neither sampling nor measurement error will be considered serious enough to bias results.

(2) If the two African data sets generate similar equations, and both equations differ from Gurr and Duvall's, then we will conclude that the general model is biased by sampling error.

(3) If one African data set generates equations corresponding to Gurr and Duvall's, and the other African data set differs, then the latter data set will be considered the most erroneous. Error in the first will be considered not serious enough to bias results, while error in the second distorts results. Sampling error by Gurr and Duvall will be concluded to be inconsequential.

(4) If both African data sets generate equations that differ from each other and from Gurr and Duvall's, then probably both sampling and measurement error are distorting results. Differences between the data sets are explained by measurement error and differences of each from the Gurr and Duvall model would be due to measurement error for Africa, as well as sampling error and/or measurement error in the 86-nation data.

Tables 6A through 6D report the relevant equations for a comparison of results across samples and across data sets. If the coefficients for the African data sets fall within the confidence bounds we will conclude that parameter estimates correspond and that the Gurr and Duvall estimate is representative. In comparing the two African data sets, however, more impressionistic criteria are employed since the African nations clearly do not constitute a probability sample. Here we will simply look for a correspondence of form, comparing the relative size and sign of coefficients.

An inspection of Table 6A leads to several conclusions concerning the effects of error on models of stress.[24] First, sampling error exists in the general model and is evident in the overestimation of the impact of wealth, the possible underestimation of the effects of past conflict,[25] the weaker effects of strain in Africa, and the appearance in both best African equations of regime institutional support. The weaker effects of economic development and strain corroborate inferences made above that these two concepts behave differently in Africa. Second, systematic error probably causes the incorporation of different concepts in the African equations. The inclusion of rebellion could reflect sampling error but most likely is caused by systematic error shared with stress. The exaggerated importance of past turmoil in all equations probably has similar roots. The similarity of the best African equations, in other words, is in part deceptive and simply may reflect the systematic error shared between rebellion and stress and between past turmoil and stress. The fact that shared systematic error between stress and past turmoil exists in only one data set, however, suggests that past turmoil has some real impact and that both sampling and measurement error are operating here. Third, error in the dependent variable, stress, drastically affects the structure of the estimated equation. Economic development and dissident institutional support are consistently unstable across data sets. Since the same measure for dissident strength is used for both data sets and since economic development was inferred to be relatively reliable, the instability is most likely caused by error in the dependent variable. The above analysis of measurement error led to the conclusion that the stress measure used by Gurr and Duvall is probably systematically erroneous; therefore, the implication is that for some variable(s) in their model the estimated coefficient is reflecting correlation with "noise" rather than with stress in a fashion similar to that of economic development and dissident institutional support in the African context. In summary, both sampling error in the general model and measurement error in the African data are damaging. Equations based on *Black Africa* data are closer to the general model, but even here results are not encouraging.

A comparison of the equations in Table 6B permits two general conclusions: (1) the sample that Gurr and Duvall utilize slightly biases their conclusions about the causes of external intervention, particularly in underestimating the impact of the primary determinant, rebellion; and (2) measurement error, while admittedly extensive for external intervention in Africa, does not result in grossly divergent results across data sets, except in the case of relating two highly erroneous variables, external intervention and turmoil.

Table 6C provides the relevant equations for turmoil. Two equations are reported for the *Black Africa* data—one employing the basic strain score and the other the strain score excluding political discrimination (see footnote 15)—since the two strain measures produce different results in the case of turmoil. Although there is some convergence between both data sets and the general model, conclusions concerning the impact of sampling error are difficult to draw because of the inclusion of traditions of conflict, which is scored differently for African countries, and tension, which includes traditions as a component. On the other hand, one would anticipate measurement error to produce considerable bias in parameter estimates given previous suggestions that error in turmoil, the dependent variable, is systematic. Attempts to generate best equations in both African data sets do produce extremely divergent results, reflecting differences in shared systematic error. On the one hand, the best equation based on Gurr data includes all of the systematically erroneous variables, and little else, whereas the best *Black Africa* equation includes only past conflict of the systematically erroneous variables. One might infer that the extent of systematic error is greater in the Gurr African data, but that the shared form of systematic error that involves past conflict most likely resides in the *Black Africa* data. The sources of remaining differences, however, seem impossible to disentangle.

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[24] Recall that Gurr and Duvall create a couple of stress scores, weighting indicators according to the structure of a regression equation predicting each form of conflict. See footnote 14.

[25] The inclusion of traditions of conflict which is scored differently for the African countries and includes as components past turmoil and past rebellion (see section on Measurement of Concepts), could explain the exaggerated effects of traditions and the weakened effects of past rebellion in the second set of equations.

The final concept explained in the general model is that of rebellion (Table 6D, where the Gurr and Duvall equation for "deaths" is also reported). Both African data sets provide results similar to the general model, and again the *Black Africa* data set produces the closest correspondence. In replications of both equations, the coefficients for the two African data sets tend to fall at the two ends of the confidence limits, suggesting that measurement error in the African data has strong effects, that the actual parameter falls somewhere in between them, and hence that the general model is relatively accurate. Measurement error is most apparent in the best African equations where turmoil and past turmoil have strong but opposite effects. Sampling error is evident, however, at several points. Both African data sets converge in the deaths equation to demonstrate the weaker effects in Africa of strain and the stronger effects of dissident strength. In addition, both best African equations incorporate dissident institutional support, rather than regime institutional support, corroborating earlier suggestions that the latter concept behaves differently in Africa and implying that dissident support is the more important determinant of rebellion in Africa.

Although the turmoil equations are perhaps too divergent to permit drawing definitive conclusions on the impact of error, equations for the other concepts provide some convergence in evidence. In all cases the *Black Africa* data produce closer correspondence to the general model, and in all cases there is some evidence of sampling distortions. The effects of measurement error appear to be strongest in the Gurr African data, and hence the decision to exclude African countries from the 86-nation study was probably wise. Moreover, certain concepts do appear to be measured with extensive error, and their inclusion in any analysis, at least of African countries, produces distortions. The most damaging form of measurement error is clearly systematic rather than random. External intervention, the most erroneous variable, has predominantly random error, but distortions resulting from its inclusion are considerably less than from the inclusion of turmoil in which systematic error predominates. The decision of the authors of *Black Africa* not to publish turmoil data in their handbook was apparently wise, since results based on turmoil data are probably too biased to produce meaningful conclusions. Given the extreme error in the turmoil data one might expect error to exist in other samples as well, and hence results based on turmoil ought to be treated with caution in any sample. Knowledge of the extent of error in individual variables, however, does not necessarily help estimate the actual effects that error will have on complex models. For instance, two reliably measured concepts—economic development and dissident institutional support—are highly unstable when related to an erroneous dependent variable such as stress.

While some data and certain concepts are so erroneous to warrant exclusion, omission of certain countries can create distortions from sampling error. Gurr and Duvall do fairly well in setting up general models of conflict (rebellion and perhaps turmoil), but their biased sample leads to unrepresentative models, especially for stress and external intervention. Exclusion of a particular type of nation—the newest and poorest—leads Gurr and Duvall to misrepresent the *general* effects of concepts such as economic development, strain, regime institutional support, and dissident strength. Thus attempts to reduce the effects of one form of error (measurement) have magnified the effects of another form (sampling).

### Error in Theoretical Specification

A third form of error which can distort model results is that of theoretical specification. The assumptions of regression analysis outlined in the previous section imply that no important variables that correlate with both the independent and the dependent variables are excluded. Their exclusion, like measurement and sampling error, will create biased estimates. Gurr and Duvall explicitly claim to be deductive, i.e., they claim that the model they generate is derived from a "theory" of conflict rather than built inductively. If their theory is good, then their model should not suffer seriously from specification error. They do, however, modify their deductively derived models in light of data on the 86 nations. Their final specification is in part determined by available data. Hence, the nature of their sample affects the model that is specified, and as a result it is difficult to disentangle specification error and sampling distortions. For example, in the previous section best descriptive equations were presented for the African sample, and several of those equations incorporate terms that are not specified by Gurr and Duvall. The descriptive equations point to possible specification errors in the general model, but the determination of whether they are specification error or distortions caused by differences in samples is impossible to make.

Thus although we are unable directly to assess specification error in the general model, it is possible to investigate the problem in the African sample to obtain some guidelines. Working only with the African sample, we can look for two types of factors possibly omitted. First, there are general factors which should be included in all models of conflict. Second, there are factors specific to Africa which should be included in models of conflict for African countries. It is possible that models of conflict appear to be different in Africa because some variable distinct to and important in Africa has not been controlled. In the language of Teune and Przeworski,[26] this problem involves translating system names (here, Africa) to variable names. In other words, if some variable were incorporated in the model for African countries, other variables would behave as they do in the general model.

Since the *Black Africa* data appear to be the least erroneous and provide results closest to the general model, the following analyses rely solely on those data. Several new variables are incorporated, and best equations are generated to assess if theoretical specification is indeed a problem. One variable, which I have dealt with previously,[27] is conceptualized as a general factor to be incorporated in any model of conflict. The concept is the institutionalization of party systems, which are viewed as linkage mechanisms between publics and governments. One could argue that where there are stable and effective linkages, conflict is less likely; rather conflict is most likely to occur where publics have inadequate channels for articulating interests to governments. Similarly, one would expect party system institutionalization to inhibit stress, since government sanctions (a major component of stress) are less necessary where linkages are strong. Party system institutionalization has been scored as a lagged variable (1955–60) and as a simultaneous variable (1961–65).[28]

Another potentially relevant general factor omitted by Gurr and Duvall relates to the effects of different forms of conflict on each other. Previous attempts to generate best African equations, as well as the following attempts, incorporate both forms of conflict in the same equation. While Gurr and Duvall do not treat one form of conflict (e.g., turmoil) as a determinant of another (e.g., rebellion), conflict is treated here as another general and theoretically important concept to be incorporated. One might expect that past turmoil might lead to later rebellion, while rebellion could inhibit turmoil.[29]

Other variables are viewed as relevant primarily to the African sample. Mother country (an indicator of the nature of colonial rule), the length of time independent, and the length of period of self-rule are all treated as predetermined variables that might be relevant in models of conflict for newly independent countries. Different colonial powers prepared governments differentially for independence, perhaps affecting later conflict and stress. On the other hand certain countries might have a greater propensity to intervene in the affairs of former colonies. One could also argue that the more mature a country (i.e., the longer self-rule or independence), the less likely are conflict, stress, and external intervention.

Table 7 reports the best equations for the *Black Africa* data set. The stress equation incorporates simultaneous party system institutionalization which apparently acts to reduce stress. Although the strength of the effect of past turmoil on stress is reduced somewhat, creating a closer corre-

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[26] Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: John Wiley, 1970).

[27] See in particular Mary Welfling, *Political Institutionalization: Comparative Analyses of African Party Systems,* Sage Professional Papers in Comparative Politics #01-041 (Beverly Hills: Sage Publications).

[28] The author has scored thirteen indicators of party system institutionalization in the black African sample for the period 1945–1970. Previous analyses demonstrated that these indicators fall into four conceptual dimensions. The highest-loading variable on each dimension was selected as representative of that dimension, and the mean standard score of the variables calculated for two time periods, 1955–60 and 1961–65. The four representative variables are percentage of seats held by independents, new party entities, electoral participation, and electoral discrimination (for postindependence years). A fuller explanation of this scoring procedure can be found in Raymond Duvall and Mary Welfling, "Social Mobilization, Political Institutionalization and Conflict in Black Africa: A Simple Dynamic Model," *Journal of Conflict Resolution* (December, 1973); and a full explanation of the scoring of all institutionalization indicators can be found in Welfling, *Political Institutionalization.*

[29] For other suggestions on how different forms of conflict interrelate see Hibbs, *Mass Political Violence,* and Duvall and Welfling.

**Table 7. Equations for Estimating the Impact of Error in Theoretical Specification**[a]

A. STRESS

Determinants of Stress

| Turlag | Rebell | R.I.S. | Inst. 61–65 | $R^2$ |
|---|---|---|---|---|
| +.45 | +.17 | −.23 | −.25 | .70 |

B. EXTERNAL INTERVENTION

Determinants of External Intervention

| Rebell | D.I.S. | Tension | Independence | $R^2$ |
|---|---|---|---|---|
| +.63 | −.30 | +.28 | −.31 | .53 |

C. TURMOIL

Determinants of Turmoil

| D.I.S. | R.I.S. | EcDev | Turlag | Reblag | Mother Country | $R^2$ |
|---|---|---|---|---|---|---|
| +.22 | −.52 | +.39 | +.69 | −.39 | −.20 | .69 |
| *Str.* *P.D.*[b]<br>+.20 −.26 | −.47 | +.38 | +.58 | −.27 | −.17 | .72 |

[a] All equations are based on *Black Africa* data for the African sample.

[b] The second Turmoil equation incorporates the Strain score which eliminates Political Discrimination. P.D. refers to the separate Political Discrimination score. See footnote 15.

spondence to the general model, the basic form of the equation continues to correspond to the best *Black Africa* equation reported in Table 6A. Party system institutionalization may well be an important general determinant of stress, since its inclusion does not render the African results comparable to the general model, and since it has rather marked effects on stress in Africa in a fashion anticipated by theory. I believe that Gurr and Duvall have misspecified in failing to account for the stress-inhibiting effects of institutionalized linkages between publics and governments.

The external intervention equation incorporates as an important variable the length of time independent (the longer an African country has been independent, the less external intervention it experiences during a particular time period). Inclusion of this variable renders the equation more comparable to the general model than was the best *Black Africa* equation reported in Table 6B. The effects of rebellion are increased, while the impact of tension, not a determinant in the general model, is reduced. The negative contribution of dissident support, however, remains exaggerated in the African sample. The length of time independent, then, seems to be a country- or region-specific variable which affects the degree to which similar processes characterize African and non-African politics.

Conflict equations appear little affected by omission of relevant concepts. None of the new variables improves the rebellion equation, although both of the best descriptive African equations in Table 6D include turmoil. The instability of turmoil coefficients caused by measurement error, however, makes it impossible to discover the "real" effects of turmoil on rebellion. On the other hand, the best African equations reported in Table 6C suggest that past rebellion may reduce future turmoil, indicating an additional failing of the Gurr-Duvall model, which treats each form of conflict as a separate and unrelated phenomenon. Finally, inclusion of mother country in the turmoil equation in Table 7 slightly alters coefficients so as to approximate the general model more closely, but changes are so slight and the increase in $R^2$ so small that inclusion of another variable is probably not warranted.

## Conclusions

The purpose of the research reported thus far has been to evaluate one effort to develop a general comparative model of conflict in light of three sources of error common to cross-national research—measurement error, sampling error, and error in theoretical specification. Conclusions are many and mixed, and advocates of diverse positions could find results here to support their cases. Careful conclusions therefore are needed to avoid extreme interpretations.

First, it is clear that the general model suffers from sampling error. An inspection of correlations across samples suggests that at least three concepts—strain, economic development, and regime institutional support—have weaker effects in Africa than in the 86-nation sample. The development of more complex models supports these suggestions and in addition provides evidence that dissident institutional support and perhaps past conflict have stronger effects in the African sample.

Second, measurement error is clearly present in the African data sets, although the *Black Africa* data appear less erroneous. The extent of error varies considerably across concepts, and both random and systematic error are inferred to exist. Measurement error does create unstable results across the two data sets, particularly when measures with systematic error are involved. Equations explaining turmoil, the most systematically erroneous variable, have no correspondence, and the inclusion of turmoil as an independent variable produces contradictory results. Equations explaining stress, a variable containing some systematic error, include unstable coefficients for what appear to be reliably measured concepts (e.g., economic development), and incorporate as independent variables those which share systematic error with stress (e.g., rebellion). Rebellion equations also differ across data sets, primarily, however, because of the inclusion of turmoil. On the other hand, distortions produced by variables with random error such as external intervention are not so great even though the error is extensive. Assuming that the inferences on the nature of error are correct, one can conclude that fairly large random error is tolerable but even small systematic error is damaging.

Third, the omission of certain potentially relevant concepts from the general model suggests some error in theoretical specification. The existence of institutionalized linkages between publics and governments seems to reduce stress, while in new nations the length of time independent influences the extent of external intervention. In addition, there was evidence that different forms of conflict affect each other, but interpretations must be extremely tentative due to measurement error in turmoil. This third form of error appears the least severe and is perhaps the most easily remedied.

Although considerable error has been found in these model-building efforts, and although errors certainly do distort parameter estimates, there are still some optimistic conclusions for the general comparativist and for the researcher involved in quantitative cross-national analysis. Some portions of the general model do hold up in the African sample, especially portions explaining rebellion, which is the heart of the model. In addition some African data appear sufficiently reliable so that the results are not distorted. *Black Africa* data provide considerable correspondence to the general model on many concepts, although certain concepts such as turmoil remain too erroneous for meaningful analyses. Two suggestions follow from this discovery. First, data collected by area specialists appear superior to data collected by large cross-national studies, at least for the African countries. Second, general comparativists could and perhaps should use these area data since they appear sufficiently reliable and their inclusion would reduce sampling error. While general models are distorted by some error basic to cross-national research, results clearly represent more than random noise, and there is some basis to believe that errors can be reduced once their nature and effects have been estimated.

# Nondecisions and Power:
# The Two Faces of Bachrach and Baratz

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Since Bachrach and Baratz published their two articles, *Two Faces of Power*,[1] and *Decisions and Nondecisions: An Analytical Framework*,[2] the concept "nondecision" has become increasingly familiar,[3] and now occupies a prominent position in the hagiology of community power. It was put forward as a means of filling what the authors saw as a gap in the conceptual armory of community studies that had resulted in a misrepresentation of American local politics. This paper looks very briefly at the limitations Bachrach and Baratz noted in the work of Robert Dahl,[4] and then at the two forms of nondecision making their solution appears to take in. The conclusion reached here is that, in its present form, the concept has no empirical value even if it may legitimately be retained to point up a weakness in the literature. But it is only part of their solution. Although "nondecision" is held to identify the gap, it is not advanced as explanation of it. In their view, this has occurred because observers have not differentiated among the various means of resolving relationships, of which power is only one form. This paper examines their proposals and concludes with a more limited view of the significance of power for empirical research.

### Dahl's Approach to Community Politics

The debate over community power has been almost exclusively an American affair. The dominant theme, understandably enough, is the attempt to come to terms, in a systematic way, with the nature of political reality in the American community.[5] The most authoritative voice in the debate thus far has been that of Robert Dahl, whose decision-making, or issue analysis, method of studying community power involves isolating selected issue areas and showing how crucial decisions are reached within those areas. Since this approach attempts to reconstruct the actual behavior of participants in various important areas of community life, it is held to provide a reliable and representative body of data about what actually goes on in the community. The pluralist conclusions drawn from this, that power tends to be dispersed among a variety of issue-oriented elites, are therefore held to be closer to the reality of community politics in America than are the findings of Floyd Hunter,[6] whose suggestion that Atlanta was dominated politically by a business elite was the spark which set the whole debate going. It was argued that Hunter's elitist conclusions could have been predicted from his reputational methodology.[7] He had, or so he thought, located the "real" holders of power by the simple and inexpensive device of asking strategically located citizens for their opinion of who was powerful, thus confusing hearsay with fact.

How far this apparent conflict is a function of methodology, and how far it reflects real differences in the communities studied, is another matter.[8] It is referred to here simply to put Dahl's work in context. Hunter's approach devalued the significance of the political process. In a putatively democratic context this was clearly a challenge. Dahl concluded his study of New Haven by arguing that

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[1] *American Political Science Review,* 56 (December, 1962), 947–952.

[2] *American Political Science Review,* 57 (September, 1963), 632–642.

[3] See for example Matthew A. Crenson, *The Un-Politics of Air Pollution: A Study of Non-Decision-making in the Cities* (Baltimore: The Johns Hopkins Press, 1971); Frederick Frey, "Comment: Issues and Nonissues in the Study of Community Power," *American Political Science Review,* 55 (December, 1971), 1081–1101; K. Newton, "Democracy, Community Power and Non-decisionmaking," *Political Studies,* 20 (December, 1972), 484–547. For a dissenting view see Raymond Wolfinger, "Non-decisions and the study of Local Politics," *American Political Science Review,* 55 (December, 1971), 1063–1080; and see also Wolfinger's "Rejoinder" in the same volume, pp. 1102–1104.

[4] Robert Dahl *Who Governs? Democracy and Power in an American Community* (New Haven: Yale University Press, 1961).

[5] See, for example, the article by Nelson W. Polsby on the "Study of Community Power," *International Encyclopaedia of the Social Sciences* (New York: Macmillan and The Free Press, 1968) III, 157–163, which he opens with the statement that "contemporary research on community power is distinguished by: (1) a concern with characterizing as a whole the political order of an entire community *(generally an American local community)*" [p. 157, emphasis added].

[6] Floyd Hunter, *Community Power Structure.* (Chapel Hill: University of North Carolina Press, 1953).

[7] Critics of Hunter are legion—see for example Nelson Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); and Arnold Rose, *The Power Structure: Political Process in American Society* (New York: Oxford University Press, 1967).

[8] Rose, pp. 296–297 notes evidence that the size of a community, and its interrelationships with the national economy, may significantly influence the nature of its power structure. This is considered in more detail by John Walton, "The Vertical Axis of Community Organization and the Structure of Power," *(Southwestern) Social Science Quarterly,* 48 (1967), 353–368, which has been most recently reprinted in *Com-*

> neither the prevailing consensus, the [democratic] creed, nor even the political system itself are immutable products of democratic ideas, beliefs, and institutions inherited from the past. For better or worse, they are always open, in some measure, to alteration through those complex processes of symbiosis and change that constitute the relations of citizens and leaders in a pluralist democracy.[9]

The central feature of the democratic political community for Dahl, then, is that the relationship between leaders and citizens guarantees the continuing possibility of change because it is the fulcrum of power. Any attempt to describe and analyze must look at this relationship within a context where both sides are motivated to act and, therefore, display the dynamics of the relationship. This can be achieved by observing the decision-making process at work. There are two difficulties which Dahl sees, at first sight, as reducing the significance of this relationship. And, of course, if its significance can be questioned, it would diminish its relevance to any description of community power.

In the first place, citizens are largely apolitical, "strongly influenced by inertia, habit, unexamined loyalties, personal attachment, emotions, transient impulses."[10] A significant political relationship is difficult to envisage where one of the presumed parties to it turns out to be so plainly lacking in interest. Dahl notes that the citizen will engage in political activity when "the actions or inactions of government may threaten . . . (his) primary goals."[11] But even though he examined three issue areas favorable to such activity in his study of New Haven, he found few signs of it. He is quite justified, it would appear, in asserting that "Homo civicus is not, by nature, a political animal."[12]

The second factor which may reduce the significance of a leader-citizen relationship, says Dahl, is the relative freedom of action available to the politician because of the complexity of the political process and the variety of interests to be considered. Thus the calculations of the politician may be critical.

> If a party politician sees no pay-off, his interest is likely to be small; if he sees an adverse effect, he will avoid the issue if he can. . . . Politicians may not see how they can gain by taking a position on an issue; action by government may seem wholly inappropriate; policies intended to cope with dissatisfaction may be blocked; solutions may be improperly designed; indeed, politicians may even find it politically profitable to maintain a shaky coalition by keeping tension and discontent alive and deflecting attention to irrelevant "solutions" or alternative issues.[13]

This sort of maneuvering is quite legitimate within a democratic context. There will be a variety of issues on which the views of constituents are confused, or conflicting. Where there is no clear lead, or support, from the public, the politician must act on the basis of his own calculations.

Yet despite the prevalence of inertia among the "apolitical stratum," and the opportunities for independent action amongst the leaders, the significance of the relationship can continue to be assumed, Dahl argues. The majority of citizens may be politically inactive, but, "although their influence is low, their indirect collective influence is high."[14] And the politicians' actions and beliefs "are all constrained by the wide adherence to the [democratic] creed that exists throughout the community."[15] Dahl plainly believes that this restraint is significant because "to reject the democratic creed is in effect to refuse to be an American."[16] So the creed inhibits the politician and causes him to attend to the wishes of the politically inert citizen, who is thus endowed with indirect influence.

Dahl does not say explicitly that New Haven is America in microcosm. He acknowledges that his "data on New Haven are not wholly adequate for the task at hand," which is a general discussion of "Stability, Change and the Democratic Creed."[17] He nevertheless asserts that "New Haven will provide a convenient reference point" without showing in what way the data are inadequate. Since he does not question the relevance of his findings to the wider context, the implication is that they may be taken as a description of it. Indeed, the innuendo in the statement "the data are *not wholly* adequate" (emphasis added) surely amounts to an assertion that, in large measure, they are. Although many observers felt that his conclusions were justified, others like Parenti,[18] Anton,[19]

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*munity Politics: A Behavioral Approach,* ed. Charles M. Bonjean, Terry Clark and Robert Lineberry (New York: The Free Press, 1971), pp. 188–97.

[9] Dahl, *Who Governs?* p. 325.

[10] *Ibid.,* pp. 90–91.

[11] *Ibid.,* p. 225.

[12] *Ibid.,* p. 225.

[13] *Ibid.,* p. 93.

[14] *Ibid.,* p. 233.

[15] *Ibid.,* p. 325.

[16] *Ibid.,* p. 317.

[17] *Ibid.,* pp. 311–325.

[18] Michael Parenti, "Power and Pluralism: A View from the Bottom," *Journal of Politics,* 32 (August, 1970), 501–530.

[19] Thomas Anton, "Power, Pluralism, and Local Politics," *Administrative Science Quarterly,* 7 (March, 1963), 425–457; "Rejoinder" *Admin. Science Quarterly,* 8 (September, 1963), 257–268.

Bachrach and Baratz,[20] and Crenson,[21] believe that they are deceptively optimistic, particularly when they are generalized from the local evidence to American society at large, where, it is clear, the possibility of change is less significant for many than their total inability to effect it.

Dahl argues, then, that statements about power in a community should be based on direct observation, and not on hearsay or supposition. But since the leader-citizen relationship is regarded as the linchpin of pluralist democracy, and the core of community politics, and since there is no significant and observable interaction between its two elements, the relationship has to be rescued by two nonobservables; the leaders' commitment to the democratic creed, and the citizens' indirect influence on the leaders. Once these are accepted as valid, however, they provide the complete justification for focusing on the actions of leaders in pursuit of various objectives in various contentious issue areas, and for ignoring the apparently settled aspects of community life as well as the aspirations of the apolitical. The viability, and value, of the leader-citizen relationship is demonstrated only in those areas determined by public action. Short of public demonstrations to the contrary, Dahl can assume that what the leaders do is more or less what the citizens want. In these terms his restricted focus is justified.

## Nondecisions

**The Response of Bachrach and Baratz.** Bachrach and Baratz believe that the significance of the relationship between leader and citizen may be considerably reduced for either of two reasons. First, when politicians are able to operate out of the public eye, they may not feel themselves bound by any abstract democratic creed, or may so interpret it that they are freely able to ignore the interests of any group of constituents, and that these instances may be more significant than politicians' formal public actions. Alternatively, the most important fact may be neither the full flowering of the leader-citizen relationship, nor the covert machinations of an elite, but the imperatives of the structural setting. Some bias that is inherent in the political process itself, or the political culture, may be more significant than the strivings of either leaders or citizens. Bachrach and Baratz argue, then, that Dahl misses two important features of political life—covert control and the mobilization of bias.

In point of fact, they do not treat these as separate forms. The distinction can be inferred from their writing, but it is nowhere made explicit. The result is that such methodological proposals as they make are uninformed by what should have been fundamental to their case. The general thrust of their writing is that covert control and mobilization of bias, however they intend these to be jointly or separately interpreted, may lead to what they call a "nondecision." They define this as "a decision that results in suppression or thwarting of a latent or manifest challenge to the values or interests of the decision maker."[22] This, they argue, is what an issue analysis approach inevitably overlooks through focusing on "concrete decisions."[23] By these one must assume they mean formal, authoritative decisions arrived at by a constitutionally recognized organization or institution.[24] They believe that such decisions reflect only one face of power.

The question that needs to be asked, then, is whether the nondecision-making concept is a useful addition to the power study vocabulary. It is also worth considering whether the criticisms prompting Bachrach and Baratz's disquisition are properly attributable to a decision-making approach, or whether they are the consequence of Dahl's own partial interests.

**Nondecisions by Covert Control.** The first objection that Bachrach and Baratz make, then, is that the "symbiotic" relationship between citizens and leaders, upon which Dahl places so much emphasis, may turn out to be one in which the leaders, or some hidden elite which in turn controls them, exercise a form of covert control over the whole process. The leaders may not need to take any account of citizen interests. They may be able to get their way by behind the scenes manipulation, thus provoking no hostile citizen reaction. The effect of this is that

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[20] Both the Bachrach and Baratz articles (see notes 1 and 2 above) have been reprinted, together with a sketchy report of an associated empirical study, in their *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970). All further Bachrach and Baratz references will be to this book.

[21] See Crenson, especially pp. 181–182 for a brief, but very penetrating analysis of the logical difficulties posed by the concept of indirect influence.

[22] Bachrach and Baratz, *Power and Poverty,* p. 44. Note that this definition of nondecisions is more restrictive than what is implied by mobilization of bias since it denies the possibility of such bias operating against "the values or interests of the decision makers." Clearly the mobilization of bias may not itself discriminate how it operates, and against whom—as is of the essence of covert control. The failure to distinguish between these two forms of nondecision making creates the confusion which lies at the heart of their approach, and is the justification for Merelman describing them as "neo-elitists." Richard Merelman, "On the Neo-Elitist Critique of Community Power," *American Political Science Review,* 62 (June, 1968), 451–460.

[23] Bachrach and Baratz, p. 9.

[24] They would appear to mean the same class of events as Polsby when he refers to "specific outcomes," and cites examples such as party nomination, urban development programme, and public education. See Polsby, *Community Power and Political Theory,* pp. 113–114.

> demands for change in the existing allocation of benefits and privileges in the community can be suffocated before they are even voiced; or kept covert; or killed before they gain access to the relevant decision-making arena; or failing all these things, maimed or destroyed in the decision-implementing stage of the policy process.[25]

Merelman dismisses this argument on the grounds that it presumes an elite in advance of any evidence that one exists. Neither misfortune, frustration, nor inequality can necessarily be taken as the work of a hidden hand—which is an implication of Bachrach and Baratz's approach. Certainly the violence of their description can be dismissed as merely tendentious. After all, the "suffocating, hiding, killing, maiming and destroying" could equally well be directed toward demands for stability in the existing allocation of benefits, where action is necessary to adjust to changing conditions. Or, as Merelman argues, "the neo-elitists formulation of non-decision-making . . . emphasizes only those values which contribute to elite control and support the *status quo.* But are there no 'dominant values,' no 'accepted rules of the game' which favor the initiators of issues?"[26]

The question can, of course, be answered only by locating those instances where this form of nondecision making actually occurs. And if the purpose of all this is a study of community power, it would require a reasonably comprehensive statement covering a communitywide range of nondecisions. Without this one can make no assertion about the community significance of any singular instance, unless one is prepared to suggest that certain nondecisions may be identified and selected for study because they are in some way representative.[27] The issue analysis approach to the study of community power faces the same problem, and resolves it by selecting what are believed to be important issues. Polsby justifies this by asking the rhetorical question," What sort of power elite asserts itself in relatively trivial matters, but is inactive or ineffective in the most significant areas of community policy-making?"[28]

Bachrach and Baratz argue, however, that "the [pluralist] model provides no *objective* criteria for distinguishing between 'important' and 'unimportant' issues arising in the political arena."[29] Yet they do not establish objective criteria either for selecting nondecisions or for identifying them. Nor do they follow the pluralists' modest precaution of studying a selection of issue areas.[30] In this respect their scheme offers no advantages over the one they criticize.

For example, it is quite clear that this first form of nondecision making, where the barrier to certain forms of action is consciously, but covertly, erected by the "*status quo* defender," is likely to be very difficult to distinguish from the calculations of politicians in confused and complex situations, and leading to inaction, such as Dahl referred to. Methodologically the two classes of events make the same demands. In both cases one is concerned with explaining something which does not necessarily have a public face. In fact, "deflecting attention to irrelevant 'solutions' or alternative issues,"[31] as Dahl puts it, is precisely, although not solely, what Bachrach and Baratz mean by nondecision making. They argue, though, that Dahl is unable to deal with this because his methodology (and, by implication, all decision-making analysis) requires him to focus on an "examination of a series of concrete cases where key decisions are made."[32] But the question is whether this limitation is a function of decision-making analysis generally, or whether it is a peculiarity of the line taken by Dahl. If it is the latter, then the answer to their problem may lie, simply, in a sensible application of decision-making analysis techniques—in which case they may have been indulging in a case of special pleading on behalf of a useless neologism.

In examining politics in New Haven, Dahl was chiefly concerned with asking, "How are important political decisions actually made?"[33] and he focused on "important decisions requiring the formal assent of local government officials."[34] In an Appendix he notes that the "three issue areas were chosen because they promised to cut across a wide variety of interests and participants . . . and in each of these issue areas all the decisions that the participants regarded as the most important since about 1950 were selected for detailed study."[35] Now it is clear that these research procedures do not necessarily follow from the initial question. While there is no denying the relevance of the issues selected, a definition of "important political decisions" does not require that a decision not to act, or a behind the scenes maneuver, or supportive, as distinct from initiatory, behavior, be ignored. That Dahl does ignore such aspects of the political scene is incontestable.[36] But this oversight was not determined by the general questions with which he prefaces his inquiry.

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[25] Bachrach and Baratz, p. 44.

[26] Merelman, p. 459.

[27] See Roy Forward, "Issue Analysis in Community Power Studies," *Australian Journal of Politics and History,* 15 (December, 1969), 26–44, for an exhaustive analysis of the varieties of issue that may occur in a community. But drawing attention to variety is not in itself enough, since the profusion of alternatives simply defeats the objective of focused analysis.

[28] Polsby, *Community Power and Political Theory,* p. 114. This question makes sense only if one is concerned with power comparability. A typical reason for such a preoccupation with *that* seems to follow from the belief that the sharing of power is a crucial index of democracy. (On this point, see L. J. Sharpe, "American Democracy Reconsidered: Part II and Conclusions," *British Journal of Political Science,* 3 (April, 1973), 129–167, at p. 135). Such an approach tends to ignore the style and content of politics. One should not only consider who governs, but how, and in relation to what.

[29] Bachrach and Baratz, p. 6.

[30] Polsby, p. 113. "More than a single issue area is always chosen . . . because of the presumption among pluralist researchers that the same pattern of decision-making is highly unlikely to reproduce itself in more than one issue area."

[31] Dahl, *Who Governs,* p. 93.

[32] Bachrach and Baratz, p. 9.

[33] Dahl, p. 7.

The reason is not hard to suggest. Dahl was, perhaps, more concerned with producing a reasoned response to the views of Hunter, who pictured an American community as controlled by big business, than with establishing a general picture of community politics. "The question is," he wrote two years after the publication of *Who Governs?*, "whether businessmen or related groups, social strata or classes, also dominate the decisions of *public* government . . . if not, what groups or strata have the most influence over the decisions of public governments."[37] He justified this particular focus by arguing that it was "the arena in which the controversy over 'community' power arises."[38] But is community power simply a function of group conflict over governmentally controlled decisions? This is as much a matter for investigation as is the question of who governs. In a critique of Dahl's study Anton complains, "Surely the study of power must involve more than the actions of government agencies."[39] Whatever one may believe to be the case, the answer needs demonstrating rather than merely asserting.

There is no reason, for example, why the decision-making analyst should not decide that the most important decision in community X, reputedly democratic, is that which has resulted in, and continues to maintain, the exclusion of forty per cent of the citizens from any effective part in the political process. This was Bachrach and Baratz's concern in examining "political interaction between the black poor in Baltimore and the white-dominated 'Establishment',"[40] and which they sought to explain by using the concept "nondecision making." But, if an observer wants to account for some problem in a community, he may do so either by reference to decisions taken, or to some process which is the embodiment of past decisions, or to the fortuitous working of circumstances. Our understanding is not advanced by putting forward "nondecision making," as if another category of possibilities existed. In this sense, then, the term "nondecision making" is superfluous.

**Nondecisions through Mobilization of Bias.** Bachrach and Baratz's first form of nondecision making, then, adds nothing to Dahl's general discussion of power. They are correct to point out that his empirical work fails to exemplify the range of his discussion, but this applies with equal force to their own efforts in this field. They add neither theoretical nor methodological perspective and are wrong to assume that any limitation in Dahl's work is justification for a new approach.

But it seems that they go further in their criticisms and argue that the relationship is also distorted by the mobilization of bias. Both leaders and citizens may be affected by this because, as they point out by quoting Schattschneider, "all forms of political organization have a bias in favor of the exploitation of some kinds of conflict and the suppression of others because *organization is the mobilization of bias.* Some issues are organized into politics while others are organized out."[41] Yet their research strategy does not suggest any means of examining or assessing the significance of a mobilization of bias. In fact, there is some confusion in their thinking here. It is easy to follow through their reference to killing, maiming, suffocating, etc., by whatever means are available to the "status quo defenders."[42] This, as noted above, can be put under the general heading of "covert control." Since this is rational, purposive, calculation it need not be connected in any way with the mobilization of bias. It is not so easy, though, to see the value of their reference to the mobilization of bias except as a general legitimation of their claim that there are more things in their political universe than are dreamt of in Dahl's philosophy.

From what they have written it appears that the mobilization of bias is itself to be considered a form of nondecision making as well as, and separately from, the actions of individuals in support of, or modifying it. For in discussing "such bias" they write, "To the extent that a person or group—consciously or unconsciously—creates or reinforces barriers to the public airing of conflicts, that person or group has power."[43] If someone does unconsciously create or reinforce a barrier, however, it is unrealistic to say that they are, for that reason, powerful. My style of speech or mannerisms may, for example, inhibit someone else's actions or opinions without my wishing this to be so. In fact it may be my wish that they should not be inhibited. If that is so it would seem rather peculiar to say that, by unconsciously accomplishing what I would not have wanted to happen, I have therefore demonstrated my power. Where my actions do have such an effect, whether I subsequently approve of the outcome or not, then presumably it is because it is customary for some to respond in that sort of way to that sort of stimulus; in which case, it is more proper to impute power to the customary basis of behavior rather than to the behavior itself. In other words, the mobilization of bias may itself be recognized as the effective agent. It is as tangible a fact of life as the overt actions of some who may exploit it for their own benefit. The question is how one makes it the subject of a research exercise.

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[34] *Ibid.*, p. 102.

[35] *Ibid.*, p. 333.

[36] See Anton, "Rejoinder," p. 265; and also Peter Morriss, "Power in New Haven: A Reassessment of 'Who Governs?'," *British Journal of Political Science*, 2 (October, 1972), 457–465, at pp. 459–460.

[37] Robert Dahl, "Reply to Anton's Power, Pluralism and Local Politics," *Administrative Science Quarterly*, 7 (September, 1963), 250–256, at p. 254.

[38] Dahl, "Reply to Anton," p. 254.

[39] Anton, "Power, Pluralism and Local Politics," p. 453.

[40] Bachrach and Baratz, p. 53.

[41] E. E. Schattsschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, Inc., 1960), p. 71; cited in Bachrach and Baratz, p. 8.

[42] Bachrach and Baratz, p. 43.

Its existence, and its significance, can be determined only by reference to some datum point since not only is organization the mobilization of bias, but so, in a sense, is civilization itself. The observer must attack the problem from a perspective which he determines himself, or which emerges from the community he chooses to study. He may thus seek to analyze community interaction in terms of some notion of justice or equality, or in terms of the pursuit of a neglected issue he nominates as of some significance. In addition he may analyze unsettled grievances held by citizens, perhaps established by a sample survey. In neither case is anything proposed here which has not already been touched on by Bachrach and Baratz, and Crenson. Either way the observer has to focus on specific aspects which are believed, on whatever grounds, to be a manifestation of bias.

Now asking the question, "Why did this, or that, *not* happen?" makes the same methodological demands as its opposite. There is, of course, the possibility that the mobilization of bias may be so pervasive, so diffuse and insidious that it simply cannot be captured in action, rule, or speech. But this resurrects, in a different form, the "principle of infinite regress" and may be safely left to the metaphysicians.[44]

**Identification of Nondecisions.** Bachrach and Baratz have argued, then, that Dahl has failed to establish sufficiently comprehensive criteria for selecting important decisions, and that this oversight leads him to ignore important aspects of community politics. Crenson, in his study *The Un-Politics of Air Pollution: A Study of Non-Decisionmaking in the Cities*, in which he first sets out a detailed case study of the differing responses of two neighboring American communities to almost identical air pollution problems, and then uses some very slender data from fifty-one cities to test the propositions thrown up by the case studies, agrees with this argument. He believes that the "power to restrict the scope of the political process . . . is not revealed by the investigation of political activities in key issue areas because the issues in which this kind of power is likely to become significant are precisely the ones that never become 'key'."[45] But "which non-events are to be regarded as significant?," Polsby wants to know.[46] Bachrach and Baratz say go and ask the locals.[47] Crenson gets round the problem by arguing that significant nonissues are—well, significant in the way that "the prevention of elephant stampedes or the persecution of witches"[48] in contemporary America is not. In other words, the researcher should use his common sense. There is everything to be said for this, but rather than pointing up the need for a new concept, it merely stresses the cruciality of defining "key issue" in a way that is not restrictive.

Crenson attempts to establish the nature of the problem more firmly by defining nonissues as topics which have not been included on a "community's political agenda."[49] These can be identified because "only when advocacy or opposition has made an appearance among community leaders will we say that a topic has taken its place on the local agenda."[50] "Community political systems," he writes, "may be 'impenetrable' where certain issues are concerned."[51] In other words, nondecision making prevents issues crossing the boundary from the nonpolitical to the political system.[52] Since Crenson's definition throws an embarrassingly large area of public discussion into the "nonissue" category, we obviously need rather more selective criteria to determine what topics are kept off the "political agenda" through nondecision making. After all, every issue, whether eventually successful or not, must go through some perinatal obscurity. We have no means of determining that the alleged nondecision has been kept out and is not, in fact, going through a lengthy process of legitimation. Drawing the political boundary in one place rather than another has the effect of creating two classes of event where there may be only one.[53]

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[43] *Ibid.*, p. 8.

[44] See Robert Dahl, "Critique of the Ruling Elite Model," *American Political Science Review*, 52 (June, 1958), 463–469; and Polsby, *Community Power and Political Theory*, p. 34.

[45] Crenson, p. 21.

[46] Polsby, p. 97.

[47] Bachrach and Baratz, pp. 47–51.

[48] Crenson, p. 26.

[49] *Ibid.*, p. 29.

[50] *Ibid.*, p. 30.

[51] *Ibid.*, p. 17.

[52] The notion of "boundary" is descriptively useful so long as one need not be precise about where it actually is. It is useful, that is, as a means of drawing attention to gross distinctions between general classes of variables. Nondecision making, however, requires that the precise boundary between political and nonpolitical be determined for it is only at this point, in Crenson's terms, that it can operate. For the difficulties of boundary definition see Samuel E. Finer, "Almond's Concept of 'The Political System': A Textual Critique,"

If we look more closely at Crenson's argument that nondecision making power can be significant only where issues do not become "key," we can recognize the weakness of the nondecision case in its present form. Since Bachrach and Baratz, and Crenson in his case studies, have all focused on what clearly have become key issues in their communities, it is plain that, in the terms of Crenson's definition, none of them can have been examining significant nondecision-making power. Both issues were, as Polsby would argue, eminently researchable by decision-making analysis techniques.[54] If Crenson wishes to establish the lexical convention of calling neglected, or nascent, topics "nonissues," there can be no objection. What is objectionable is the claim that this provides a new and empirically viable form of political analysis.

### Power

The concept "nondecision" does not provide any new means of identifying and studying the less apparent features of community life that Dahl has been accused of ignoring. It offers nothing that is not already provided by a decision-making approach. But that is, of course, merely a method of inquiry. In examining power in the community[55] the kinds of data collected should be determined by our understanding of the concept "power" and not by the methodology employed.

One can at least say of the reputational approach to the study of community power that it starts from a fairly clear idea of both those linked concepts. Power, for example, involves "the acts of men going about the business of moving other men to act in relation to themselves or in relation to organic or inorganic things."[56] And community is the complex of socioeconomic relationships within which such a faculty is sustained. Polsby correctly points out that such a view presupposes that "power is a subsidiary aspect of the community's social structure."[57] It is an attempt, nevertheless, to relate political activity to its community environment. "The first and most basic presupposition of the pluralist approach," writes Polsby, however, "is that nothing categorical can be assumed about power in any community."[58] Dahl assumes, instead, the priority of rational self-interest within the vaguely limiting framework of the democratic creed. His approach to power, for example, is that "*A* has power over *B* to the extent that he can get *B* to do something that *B* would not otherwise do."[59] The effect of this approach, since it focuses on the act of individual choice, is to imply that such choice is the most significant aspect of the polity. The community exists only as "an aggregation of individuals."[60] As much may be said of Crenson who accepts the pluralist view of power[61] and ignores community entirely. Bachrach and Baratz have not sought to come to terms with the community

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*Government and Opposition*, 5 (Winter, 1969–70), 3–21.

[53] It is, of course, a perfectly legitimate undertaking to inquire why a community did not do this or that. If it can be shown that inaction was intentional, then it concerns community power. But the result would be a community study only in the most limited sense. Crenson's critique of the pluralist position, particularly of the muddy notion of indirect influence, is extremely well argued. And he displays considerable ingenuity in his study of factors influencing the pollution issue in fifty-one cities. But since these cities are reduced to little more than statistical artifacts, he entirely disposes of the community baby with the pluralist bath water. Of course, in one sense, his is not a community study. But it is so clearly advanced as part of a continuing debate on community power, seeking to validate the current focal concept of that debate, that it must be judged in terms of its contribution in that direction, and not simply on the more limited terms of its subtitle, i.e., "Non-Decisionmaking in the Cities."

[54] "Insofar as . . . goals are in some way explicitly pursued by people in the community, the method of study used in New Haven has a reasonable chance of capturing them." Polsby, *Community Power and Political Theory*, p. 97.

[55] For sociologists, community has been a key concept, although Colin Bell and Howard Newby, *Community Studies* (London: Allen and Unwin, 1971), point to a current disenchantment with the term (pp. 48–53). Political scientists have not, however, tended to regard this as a problem requiring much attention. Polsby, "Study of Community Power," p. 157, notes that community power researchers have adopted a conventionl perspective by defining community "as a population living within legally established city limits." This view was also taken by L. T. Hobhouse, *Social Development* (London: Allen and Unwin, 1924) who regarded "all populations living under a common rule as political communities, though they have only the bare bones of a common life" (pp. 41-42). Since "community implies having something in common" (Ronald Frankenberg, *Communities in Britain: Social Life in Town and Country* [London: Penguin, 1966], p. 238), it seems reasonable to take the common element as the defining characteristic.

[56] Hunter, *Community Power Structure*, pp. 2–3.

[57] Polsby, p. 7.

[58] *Ibid.*, p. 113.

[59] Robert Dahl, "The Concept of Power," *Behavioral Science*, 2 (July, 1957), 201–215, at pp. 202–203.

[60] Anton, "Power, Pluralism and Local Politics," p. 445.

[61] Crenson, p. 34.

concept. But they have made an elaborate attempt to resolve the power problem, an attempt which falls between the two extremes offered by Hunter and Dahl. It avoids putting forward definitions at the community level of generality, but goes beyond Dahl's focus on the act of power by insisting that there are a variety of types of relationship which shape the character of the community. They argue that the failure to recognize this variety has caused observers to overlook what is implicit in their general notion of nondecision making.

**The Bachrach and Baratz Proposals.** In examining the two faces of power, Bachrach and Baratz argue, one must distinguish among power, authority, influence, force and manipulation, because "while decision-making frequently does involve power relationships, it very often does not."[62] Now, the point of interest here does not lie in the definitions that they create to sustain these differences, but how these are to be applied in an empirical study. For this purpose Bachrach and Baratz specifically refer to several indicators, all of them positively associated with power, and either positively or negatively associated with the other concepts. The conditions that must be present, for example, before they would agree to the use of the concept "power" to describe the means of arriving at some conclusion to a situation would be as follows.

(1) There is a conflict of values between *A* and *B*;
(2) *A* threatens *B* with severe sanctions in the event of *B*'s noncompliance;
(3) *A*'s demand, and the severity of the sanctions, are rationally perceived by *B*; and
(4) *A* gets his way with *B*'s compliance.

They feel impelled, however, to make explicit the most general feature of power from which these more specific conditions follow. Power, they argue, is a relational concept.[63] The consequences of drawing particular attention to relationality will be discussed after the value of each of these four indicators—conflict, sanctions, rationality, and compliance—has been examined briefly.

(i) *Conflict*. The first question that needs asking is whether we should be concerned only with the overt signs of conflict, the public words and actions that express an opposition of interests; or whether we should also include the psychological state, the mere awareness of opposed interests. Bachrach and Baratz include both in their scheme; "a power struggle exists, overtly or covertly, either when both sets of contestants are aware of its existence or when only the less powerful party is aware of it."[64] But, having made the general statement, Bachrach and Baratz do not satisfactorily answer the second question that needs asking. How does one then identify conflict? Instead, in a section misleadingly entitled "Empirical Identification of Nondecisions," they shift the question to "what persons or groups in the community are especially disfavored under the existing distribution of benefits and privileges?"[65] This requires an entirely different set of questions being asked about the scope and purposes of government. Thus they leave the matter unresolved. The final question to be put is whether conflict is a necessary indicator of power. This can be best answered by posing a counter question. Do Bachrach and Baratz mean to say that power may not be exerted to prevent conflict? Merely asking the question suggests the inadequacy of such a view, therefore this first indicator of power may be summarily dismissed.

(ii) *Sanctions*. Bachrach and Baratz do not make it clear if they intend to place their several concepts on some notional "sanctions scale," ranging from the case of manipulation (to which sanctions are held to be not relevant), through influence and authority where there are "no severe sanctions," and power, where the "threat of severe sanctions" applies, to the case of force which requires the actual "application of severe sanctions."[66] Elsewhere they imply that influence involves no sanctions at all. For example, "the exercise of power depends upon potential sanctions, while the exercise of influence does not."[67] In their discussion of authority no reference is made to the subject.[68]

In view of their emphasis on severity, however, it is reasonable to assume that they mean to imply a hierarchy of sanctions. But this raises the whole question of establishing an acceptable "severity scale" against which the interpretation of the sanctions by the "patient" in the power relationship can be measured. This is crucial to their design since severity of sanction is the only significant means of distinguishing power from authority and influence.[69]

Dahl discusses this point in these terms. "Exactly what constitutes a 'severe' loss or deprivation is, to be sure, somewhat arbitrary. No doubt what a person regards as severe varies a good deal with his experiences, culture, bodily conditions, and so on. Nevertheless, probably among all peoples exile, imprisonment and death would be considered severe punishments."[70] If such an approach, with its disarming "somewhat," "no doubt," and "probably," is of any use, it certainly gives no help to the student of community power. But it is the sort of limp conclusion that one is driven to if one insists on a necessary connection between power and severity of sanction. It also places an unjustifiable limitation on the way we use the word "power." For if *A* is able to get his way without resorting to such sanctions, it would be decidedly anomalous to say that he was, therefore, not powerful.[71]

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[62] Bachrach and Baratz, p. 42.
[63] *Ibid.*, pp. 19–21.
[64] *Ibid.*, p. 50.
[65] *Ibid.*, p. 50.
[66] These references are all to Bachrach and Baratz, p. 37, Table 1.
[67] *Ibid.*, pp. 30–31.
[68] *Ibid.*, pp. 32–36.
[69] *Ibid.*, p. 37.

(iii) *Rational Perception.* Bachrach and Baratz maintain that

> power has a rational attribute: for it to exist, the person threatened must comprehend the alternatives which face him. . . . In a situation involving power, *B* is rational in the sense that he chooses compliance instead of defiance because it seems the less of two evils. In a situation involving authority, *B* complies because he recognizes that the command is reasonable in terms of his own values; in other words, *B* defers to *A*, not because he fears severe deprivations, but because his decision can be rationalized.[72]

As far as influence is concerned (the only other concept characterized by rationality), Bachrach and Baratz are less helpful. They do state explicitly that "power and influence are alike in that each has both rational and relational attributes."[73] But we are left to infer the nature of such rationality from the example of "the ambitious young man who submits unhappily to the every dictate of his rich uncle . . . because he admires wealthy men (influence)."[74]

They are arguing, then, that rationality is a sufficiently notable characteristic of these three concepts to merit particular mention. But consider the significance of what they are saying. In the case of a "power relationship" *B* is rational because he is dominated by fear; in the case of authority *B* enjoys the vicarious gratification of cherished values and is, therefore, rational; while rationality in their example of influence seems to mean no more than abject self-abasement. The resolution of a situation in any of these terms is held to be rational, whereas resolution by force is not because, presumably, the act of force does not depend for its success on *B*'s perception.

Notice that, in the case of power, authority, and influence, *B* is alleged to be rational because he has chosen to comply. There is an opportunity after the stimulus for *B* to behave in whichever way he wishes; but there must, by definition, be some limitation on this freedom implicit in the initial stimulus. The rationality of subsequent behavior is, therefore, relative to the restricted definition of the situation contained in the stimulus. In the case of power, authority, and influence, however, *B* has no control over the stimulus, whereas in the case of force the stimulus is, in a sense, *B*'s choice. It is *B*'s definition of the situation in as real a sense as agreement is *B*'s choice in the case of the other concepts. To deny rationality in this case, while claiming it for any emotional and submissive response, is highly tendentious.

The purpose of this brief discussion, however, is not only to demonstrate that Bachrach and Baratz use the term "rational" in a rather inadequate manner, but also to suggest that there is little point in trying to base a definition of rationality on the subjective state of the particular actors in a specific situation, because there are, then, no valid grounds for distinguishing between rational and irrational behavior. Yet as Frohock points out, "If criteria are introduced to allow the rational-irrational distinction, then the analytic construct gets away from that which it is trying to explain, which is the way in which actors ascribe meaning to what they do."[75] To specify rationality as a condition of power is unnecessary, then, because it raises greater problems than it may solve.

(iv) *Compliance.* One of the requirements that must be borne in mind in creating definitions of abstract concepts is how far the terms of the definition vary from conventional usage. A definition may, of course, be as arbitrary as the special purposes of the analyst require. But the more commonplace the concept, the less such semantic deviance is acceptable. Power is a commonplace. It is part of this commonplace to recognize that a man who habitually gets his way is powerful. One does not delay such recognition for the testimony of compliant former opponents. It is, thus, a further limitation on power to suppose that it can apply only to those cases where the patient is compliant. This would reduce success, which we intuitively suppose to be the chief feature of power, to a subordinate status.

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[70] Robert Dahl, *Modern Political Analysis,* 2nd ed. (Englewood Cliffs, New Jersey: Prentice Hall, 1970), pp. 32–33.

[71] We continue to associate power with the ability to inflict severe sanctions, but it is not a necessary association. As Anthony de Crespigny, "Power and its Forms," *Political Studies,* 16 (June, 1968), 192–205, points out, "If it is wished to make 'power' a technical term in the social sciences, it must be stripped of its dyslogistic associations. It must be used without any limitations concerning the ways in which power may be said to be exercised" (p. 193). This analysis has benefited considerably from de Crespigny's preliminary discussion of power, although his major concern, to distinguish between various types of power, is not felt to advance the cause of empirical political analysis.

[72] Bachrach and Baratz, pp. 22 and 34.

[73] *Ibid.,* p. 30.

[74] *Ibid.,* p. 31.

[75] Frederick Frohock, *The Nature of Political Inquiry* (Homewood, Illinois: The Dorsey Press, 1967), p. 137.

If compliance is made into a condition, then it follows that power is a term describing a whole developing sequence, spread over whatever period of time, whose two boundary points, conflict and compliance, are separated by threat of severe sanctions and rational perception of these. This is unwieldy, particularly since it requires that the power relationship ceases to exist, for lack of conflict, the moment the patient complies with a command. It creates the logically difficult position that compliance both creates a power relationship and terminates it.

(v) *Relationality*. The foregoing discussion has indicated the difficulties raised by these indicators of power. More than this there is no sign that they are specifically directed toward the initial problem, which involved an attempted clarification of what Bachrach and Baratz describe as the "Two Faces of Power." They have built these concepts on a set of terms which describe an individual state rather than a political relationship—hence the need to emphasize relationality.

The objection to such an emphasis is, quite simply, that there is no concept in the social sciences that is not relational in some sense. Sociopolitical reality cannot be conceived in terms other than those which involve, pertain to, or imply, a relationship. It is therefore, quite worthless to propose relationality as a *distinguishing* characteristic. Bachrach and Baratz argue that manipulation does not involve a relationship.[76] But all they can mean by this is that they believe a relationship exists only when both parties are conscious of each other, and also of the terms which are held to constitute the relationship. Such symmetry is, however, neither necessary to comply with conventional usage, which would not insist on a direct link being established, nor reasonable in the demands it makes for empirical analysis. Relationality is emphasized, of course, as a rebuttal of the view that power is a commodity that can be possessed by one person outside the context of any relationship. But why go to such lengths to refute what is, after all, irrefutable, and equally undemonstrable?

It is, then, impossible to regard relationality as a characteristic of specific "power events" in an otherwise nonrelational political field. This latter is, to steal a phrase from Bentham, "nonsense on stilts." Bachrach and Baratz argue that one must "distinguish clearly between power over people and power over matter."[77] They overlook the rather banal point that the political scientist does so by choosing not to study matter but people in their political relationships.

**Intention and Effect: The Contextual Alternative.** If these unnecessary associations are removed from the discussion of power, we are left with an agent and an effect, and the presumption that the agent intended the effect. In this view, then, power is simply "the production of intended effects."[78] If a term is used to describe an effect, however, it cannot also be used to explain its cause. For example, while we use the word "bang" to describe the effect of detonating an explosive, only a child would use the same word to describe the explosive itself. We develop the ability to distinguish between cause and effect. There is no reason to suppose that power should be treated any differently. If it is, then one is faced with the question of what creates power, and the answer would have to be power. In which case one would be driven to conclude that power is an inherent property of the power resource, which is an absurdity. Although we say, for example, that money is power, the expression is clearly an ellipsis. We know quite well that some other agency must intervene before the connection is made. This would be either an individual who actively uses money as a resource, or the institutionalization of money which may result in others responding to what they believe to be the wishes of the wealthy without any overt prompting to do so by the wealthy themselves. Money is, potentially at least, power. But power potential cannot be the same as power, by definition.[79] If we continue using the same word to describe both we confound understanding by obscuring what is to be explained.

Any approach to power which ignores the distinction between cause and effect, then, is bound to lead to difficulties. But in conventional usage we frequently describe someone as powerful outside the context of any particular situation. We may thus describe an agent as powerful where known to have been successful in the past, and believed to retain the capacity for success. We would say that he "possesses" power, or "is powerful." In his case, we would imply, power is a cause of success, and it is because of this that he can get his way. Yet such an imputation is justified by past actions, or by our ready assumption of what, in those circumstances, ought to constitute power. We could not apply these terms to anyone who, although possessing all the objectively appropriate resources, has never appeared to have been intentionally successful.[80]

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[76] Bachrach and Baratz, p. 37.

[77] *Ibid.*, p. 19.

[78] Bertrand Russell, *Power: A New Social Analysis* (London: Allen and Unwin, 1938), p. 35.

[79] See Rose, *The Power Structure*, pp. 45–53.

[80] See de Crespigny, pp. 192–196.

So although conventional usage refers to power as cause as well as effect, it does so only where there appears to be some known, or assumed, record of success. It is a convenient simplification where one does not know, or one does not choose to spell out, why such success has been possible. That general usage will be retained here. Power may be variously referred to as being created, sought after, possessed, exercised, structured, eroded, fragmented or lost. Nothing more than the processes related to the "production of intended effects" is implied. Power, then, has the same limited, but graphic, value as "bang." Recognizing the value of a term which points up the salient features of our concern does not mean that we should be led into believing that that term is sufficient explanation in itself. It is more important to consider the context in which power may be discussed.

One could argue that this is little different from the approach adopted by Dahl, which also isolates a reference point and attempts to encapsulate it within a wide range of explanatory data.[81] However, the similarity should not mask the divergent consequences for empirical analysis.

Dahl's approach to power is that "*A* has power over *B* to the extent that he can get *B* to do something that *B* would not otherwise do."[82] If this is adopted as a guide to research, one must first identify two parties in interaction, and then establish a change of position by either participant which can be attributed to the efforts of the other. The inevitable result is that one must focus on the dynamics of a concretely verifiable relationship. It is not possible for such an approach to deal with the problem of inaction, which is just as relevant to the discussion of power.

The definition adopted here shifts the focus instead to the *intention* to produce an effect, as well as the *effect* itself. It does not require that movement be demonstrated in the position of any other party. This means that empirical analysis is freed from a preoccupation with the intricacies of an ascertainable relationship. It means also that the intentions of actors in the community, institutional as well as individual, must be given equal status with the problem of effects—which has, so far, received the major emphasis.[83] This implies, of course, that one must establish a causal relationship between intention and effect before saying with any certainty that one has identified an instance of power. While this may be an ideal position to work toward, one can never demonstrate conclusively in the social sciences that any given factor is both necessary and sufficient for the production of any given effect. This difficulty indicates the importance of circumstantial evidence. It is more useful to formulate an approach which deals with the context of power than to attempt to capture power *in flagrante delicto*, although one naturally anticipates that specific conclusions may be drawn from such a general account.

**Conclusion.** This discussion suggests that the concept "nondecision" is of no practical value in the analysis of community power, because it masks the useful distinction that can be made between covert control and mobilization of bias, and offers nothing which is not already available through decision-making analysis. The problem it was directed toward can be resolved only by adopting a more modest approach to the concept "power" which does not require the observer to focus on individual initiatives within the context of an observable relationship. Such an unnecessary restriction devalues the significance of the context to the description of power. It is only by capturing something of this that the observer can hope to understand the forces shaping the relationships that constitute the community. There is no shortcut to be gained by elaborating a set of stipulative definitions, as Bachrach and Baratz have attempted. The result is merely to inhibit the observer's sensitivity to his surroundings. Since their work can be taken primarily as an appeal to this, it is unfortunate that the "Two Faces" they have put on their concern should both look inward onto a definitional maze.

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[81] I am grateful to Alan Alexander of Reading University for his forceful probing on this point.

[82] Dahl, "Concept of Power," pp. 202–203.

[83] A definition of "intentions" and "effects" poses no more problems than does a definition of "issue" or "decision." In both cases the problem of establishing a requisite level of significance arises, but can be dealt with only by the observer using stated criteria within the context of a specific study.

# Power and Its Two Faces Revisited: A Reply to Geoffrey Debnam

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In a pair of articles published a dozen years ago and more extensively in a book-length essay that appeared in 1970, we expanded the then-existing analytical model for studying the political process (defined to include nongovernmental as well as governmental actors and institutions).[1] Ruthlessly oversimplified and somewhat abbreviated, these were the assumptions and hypotheses that constituted our conceptual framework:

> (a) In most polities, large and small, the distribution of benefits and privileges—notably, income, wealth, power, social status—is highly unequal among persons and groups.
>
> (b) Over a period of time, inequality is sustained, in its essentials if not entirely, by the "mobilization of bias," that is, the prevailing norms, precedents, myths, rituals, institutions and procedures that operate "in favor of the exploitation of some kinds of conflict and the suppression of others. . . . "[2] In other words, although those institutions and procedures appear to be neutral as between those seeking and those resisting distributional change, it actually regulates the nature and extent of conflict.
>
> (c) The use of power and its correlates is a crucial means for sustaining and strengthening the mobilization of bias and thereby perpetuating "unfair shares" in the allocation of benefits and privileges. The exercise of power towards this end is a major form of nondecision making, defined as a process for thwarting latent or manifest challenges to things-as-they-are.
>
> (d) Nondecision making, which is exercised both overtly and covertly, can take several other forms. The most direct and extreme is resort to force. Just as direct but less extreme is exercise of power against would-be change-seekers, ranging from intimidation (potential deprivation of valued things) to co-option (potential rewards). The indirect forms of nondecision making, however, are the most important: utilization of an existing bias of the political system, such as a norm, precedent, rule or procedure; or reshaping and strengthening the mobilization of bias as a whole.

### The Concept of Nondecision Making

Although these ideas have received warm endorsement from many sources, they have also attracted strong criticism. Particularly sharp dissents have been entered by Richard Merelman and Raymond Wolfinger.[3] They are now joined by Geoffrey Debnam.

In his critique of nondecision making, Debnam does little more than echo his two predecessors. He concedes that the concept "is descriptively suggestive of certain possible areas of neglect" in investigations of community power, but like them contends that "it has not been shaped into a useful analytic tool. . . . " Like them, he asserts that nonevents and nonissues are unresearchable. And like them, he concludes that nondecision making creates no demands "which cannot be met by decision-making analysis."

In view of Debnam's virtual redundancy, why do we respond? We do so mainly because we are impressed and depressed by the failure of yet another competent scholar to recognize, let alone grasp, the *theoretical* significance of the nondecision-making concept in the larger context of explaining the nature and uses of power. As to this, two features of our concept are central. The first is best expressed by the major assumption upon which the concept is grounded: in a reasonably stable polity, power is mainly exercised not by those who make decisions nor by those who decide agendas, but by persons and groups who direct their energy to shaping or reinforcing predominant norms, precedents, myths, institutions, and procedures that undergird and characterize the political process. What this means, among other things, is that analysis of nondecisions is predicated on the assumptions that power is a crucial variable and that in studying power, the focus should be upon whether or how it is employed to sustain or modify the most fundamental aspects of political institutions and processes. The decision-making model, in stark contrast, incorporates the basic premise that norms, precedents, and institutions are "*givens*" within which power relationships among actors (e.g., construction of decisional agendas, participation in issues-resolution) are to be studied.

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[1] Peter Bachrach and Morton S. Baratz, "Two Faces of Power," *American Political Science Review,* 56 (December, 1962), 947–952; "Decisions and Nondecisions: An Analytical Framework," *American Political Science Review,* 57 (September, 1963), 632–642; and *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970).

[2] E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960), p. 71.

[3] Richard M. Merelman, "On the Neo-Elitist Critique of Community Power," *American Political Science Review,* 62 (June, 1968), 451–460; and Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," *American Political Science Review,* 65 (December, 1971), 1063–1080. Our brief commentary on Merelman, together with his reply, appear in *American Political Science Review,* 62 (December, 1968), 1268–1269.

Nondecision making in a power context is based on the additional presupposition that political consensus is commonly shaped by status-quo defenders, exercising their power resources, and operates to prevent challenges to their values and interests. Again in sharp contrast, the decision-making model is built on the assumption that consensus is the result of unguided molding of diverse attitudes and beliefs of a large share of the population and therefore constitutes no significant barrier to entry of important grievances into the decisional process. By declining to regard the consensual area of politics as a "given" and treating it instead as a variable that is modifiable through exercise of power and its correlates, we add an important dimension to study of the political process. In particular, our approach broadens the definition of "political interest" so that it includes overt and covert grievances, as well as challenges to things-as-they-are that have actually become issues.[4] In this way, yet another vital area of politics is opened to examination from a perspective that brings into focus *both* faces of power.

The differences between us and Merelman, Wolfinger, and Debnam are epistemological as well as conceptual. All three of our severe critics have been able to demonstrate, at least to their own satisfaction, that empirical verification of nondecision making is difficult, if not impossible. But none has been able to demolish our proposition that this second face of power is actually exercised in the real world; witness Debnam's concession that it "is descriptively suggestive of certain possible areas of neglect. . . . " Unrelenting empiricists all, they find themselves in a tight corner. They must either pretend that there is no "second face" of power, limiting the scope of their inquiry to what is empirically determinable and thereby running the risk that they will seriously misrepresent reality. Alternatively, they can claim, as Debnam does, that (1) nondecision making is simply covert control; (2) covert control can be analyzed in the same manner as overt control; and therefore (3) nondecision making is not a distinctive phenomenon and can be discarded on the ground of redundancy. The errors here, as we shall stress below, are that Debnam seriously misconstrues the nature of nondecision making and, by trying to rule out the phenomenon also asks empirical investigators to run the risk of seriously misrepresenting reality.

The way out of this false dilemma is obvious, however distasteful to the empiricists. It is to adopt an analytical model that incorporates hypotheses which, while difficult to verify empirically, compel the investigator to explore a broader range of aspects of the polity within a power context. The product of the research may well be impressionistic or ill-supported by data, but better this than compounding the error by ignoring altogether the elusive elements. To make the point in more general terms, while we recognize that theoretical propositions must always be tested by reference to verifiable data, we also insist that theory is another important form of knowledge that must be encouraged to produce new, expanded, fruitful ways to understand politics. It is a reasonable expectation that as it performs this task, it will spur empiricists to devise appropriate techniques to test its validity.

### Forms of Nondecision Making

Debnam's critique of our works suffers additionally from his serious misunderstanding of the forms of nondecision making. Because others may share his confusion, we now address his mistaken inference that there are two major forms of power's second face—covert control and mobilization of bias.

The notion of covert control is both too broad and too narrow to qualify as a form of nondecision making. It is too broad because *both* faces of power can be exercised covertly. For example, a legislator may be bribed covertly as a means of keeping a grievance from becoming a political issue (nondecision making) *or* to assure victory in the decision-making arena. The same applies to covert threats of blackmail, physical violence, political threats, and the like. These covert strategies may be used either to prevent issues from surfacing (nondecision making) or winning issues once they have surfaced (decision making). It is too narrow because there are numerous ways in which nondecision making is overt; among several illustrations we gave in *Power and Poverty* (pp. 44–45) are co-option of opponents, public denial of legitimacy to demands for change, and deflection of challenges by referring grievances to study commissions.

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[4] We suspect that our conception of "interest" is still too narrowly drawn to permit a thorough analysis of consensus in circumstances where there appears to be *no* dissent in a polity, overt or covert. Perhaps, as Matthew A. Crenson suggests in his first-rate comparative study of nondecision making (*The Un-politics of Air Pollution: A Study of Non-Decision-making in Two Cities* [Baltimore: The Johns Hopkins Press, 1971]) and as Steven Lukes argues in his brilliant essay, *Power: A Radical View* (London: Macmillan, 1974), our error is to rely upon a subjective conception of interest. For a discussion of this issue as it relates to "false" consciousness, see Isaac D. Balbus, "The Concept of Interest in Pluralist and Marxist Analysis," *Politics and Society,* 1 (February, 1971), pp. 151–177; William E. Connolly, *The Terms of Political Discourse* (Lexington: D. C. Heath, 1974), pp. 45–85; and Peter Bachrach, "Interest, Participation and Democratic Theory," in *Participation*, ed. Roland Pennock and John Chapman (New York: Atherton Press, 1975).

Our concern over Debnam's error here goes well beyond annoyance at his slovenly reading and analysis of our work. By construing nondecision making as covert control, he carefully sets up the further argument that the concept is redundant. What else can be concluded from his subsequent assertion that by "a sensible application of the techniques of decision-making analysis" the investigator can observe decisions—and thus the exercise of power and its correlates—on the covert level of politics as well as in the formal decision-making arena?

The allegation of redundancy would indeed be valid if the two faces of power on the covert level were used for the same purposes. They are not. Nondecision making serves a radically different set of purposes than does decision making, and on both levels of the political process. Debnam's failure to catch this crucial distinction is matched by his inability to explain satisfactorily how decision-making analysis can capture those "possible areas of neglect" of which nondecision making is "descriptively suggestive."

What is especially puzzling in this respect is his lack of concern about how the techniques of decision-making analysis can adequately serve to identify nondecisions, when the concepts of decision making and nondecision making are grounded upon such different and conflicting assumptions—assumptions pertaining to the scope and openness of the political arena, the genuineness of consensus, the neutrality of political institutions, and the concept of interest. Turning his back on such theoretical concerns, he is content to assert grandly but vaguely that "if an observer has reason to believe that a problem exists in a community, then it is a modest assumption that an example of that problem will produce some significant evidence concerning the nature of power in that community."

The simplicity of this approach is beguiling. But in the absence of some conceptual framework that provides an adequate basis for observing the interaction of the two dimensions of power with political institutions and prevailing social values, the analyst will be only partially equipped, limited to studying only the "first face" of power. Under Debnam's approach, how is the observer to determine that the problem exists because of a decision, as opposed to one form or more of what we define as nondecisions? On these questions Debnam is silent, apparently asking us to have faith that he knows what to examine and how, and that in the process he will produce some significant evidence about power.

No less distressing is his erroneous conclusion that we view the mobilization of bias as a form of nondecision making. In very plain language, we said (*Power and Poverty*, p. 44), "The primary method for sustaining a given mobilization of bias is nondecision-making." We went on, there and subsequently, to distinguish carefully between those who exercise power to create, shape or reinforce the mobilization of bias and the mobilization of bias itself, secure in the understanding that failure to make the distinction could lead to the error of focusing solely upon the mobilization of bias to the exclusion of actors who exercise power and its correlates to sustain the mobilization and make it politically effective. Here again, however, we suspect that Debnam's misreading was more deliberate than accidental. By enveloping the exercise of power within the mobilization of bias, he eliminates in his own mind the interaction between actors and the mobilization of bias and, in doing so, permits the conclusion that nondecisions (at least in this form) are non-events. The way is then open for him to ask, quoting Nelson Polsby, "Which non-events are to be regarded as significant?"

Two points must be reiterated before addressing this question. First, nondecision *making* (almost invariably, Debnam and others of like mind speak of "nondecisions," a noun, rather than "nondecision making," a gerund that connotes active exercise of power) is an act, performed either overtly or covertly, that is susceptible to observation and analysis.[5] Second, the mobilization of bias is an effective instrument of power and is also observable in its nature and impact; true, it is not an "event," but its existence is demonstrable both by reference to its impact upon those who wish to transform their grievances into issues and by reference to its linkage to power wielders (nondecision makers) who shape and support it. The observability of both nondecision making and the mobilization of bias, in other words, is materially assisted by their interaction and their direct or indirect effect upon potential issue raisers.

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[5] Debnam's argument on this point is contradictory. On one hand, he contends acts of this kind can be observed through use of techniques of decision-making analysis. Yet, on the other hand, he characterizes such acts as nonevents and therefore nonobservable.

To return to the question at hand, the criterion or criteria for determining if a nondecision is significant, we have answered straightforwardly: A potentially key issue "is one that involves a genuine challenge to the resources of power or authority of those who currently dominate the process by which policy outputs in the system are determined" (*Power and Poverty*, pp. 47–48). Can potentially key issues, so defined, be identified empirically? And can the same be said about nondecision making and the mobilization of bias?

Our research in Baltimore is not the only evidence in support of affirmative answers to these questions. We cite additionally the excellent study by Verba and Nie, which exemplifies the feasibility of identifying a comparatively subtle form of mobilization of bias, coupled with supportive nondecision making within a large polity.[6]

The study confirms the standard socioeconomic model of political participation: upper-status citizens—the more affluent, educated, and prestigious—are significantly more active politically than other members of the polity. The participatory gap between social classes is increased by other forces which the study details. Among the findings is that those who most need to participate in the political process are the nonparticipants. Because they are for all practical purposes outside the process, they have little or no influence either in shaping policy or, more importantly, in determining which issues reach the decision-making arena. Those who do set the agenda have interests that are markedly different from those of nonparticipants.[7]

Verba and Nie interpret these findings as evidence that participatory institutions and rights in the United States, although unequally utilized, are politically neutral. "Equal rights to participate," the authors observe, "are just that: *rights*, not obligations. Some may take advantage of these rights, others may not."[8] They even claim that "participation remains a powerful social force for increasing or *decreasing* inequalities. It depends on who takes advantage of it."[9]

Viewed from our perspective, however, a different interpretation is possible. If rights to participate are considered, not in abstract terms, but by reference to the way in which *they are actually used, by whom, and with what effects over time*, it is possible to characterize the existing participatory system as class-biased. The belief in the system's neutrality precludes open consideration of alternatives to it, such as proposals for workers' control of industry and neighborhood governments. Put differently, so long as the existing participatory system's legitimacy remains unimpugned, grievances about it will fail to get on the decisional agenda and changes in it will fail to be made.

Verba and Nie's work is defective to the extent that it overlooks this critical point. What is more important, their neglect contributes in a small but nontrivial way to reinforcement of the mobilization of bias in favor of the established participatory system. Thus, although they were probably unconscious of doing so, they were engaging in a form of nondecision making.

## The Meaning of Power and Its Correlates

Debnam devotes the concluding section to an extended disquisition on our definitions of power and its correlates. In doing so, he has read much more into our definitional exercise than we intended. Our purpose was simply to distinguish rigorously among a group of concepts that are generally regarded as synonymous, in the interest of a greater degree of analytical and expository precision. The importance of doing this is underlined by Debnam's misapprehension, for instance, that "severity of sanction is the only significant means of distinguishing power from authority and influence." We labored—evidently without success in Debnam's case—to make the point that sanctions are not invoked at all in situations involving authority and influence, whereas in power relationships sanctions are threatened but not invoked.

We certainly did not mean, as Debnam charges, that political analysts should spend their time, with our little lexicon at hand, separating and dissecting the attributes of power and its correlates. That would indeed be a waste of time. Nowhere in our writing, expressly or by implication, did we subscribe to such an approach. To the contrary, all of our definitional efforts were carefully cast in a broad political context. We concede, however, the validity of one of his criticisms of our conception of power: it is more relevant to an individual state than to a political relationship. Ironically, the same criticism can be leveled at Debnam's conception of power, which focuses upon "the *intention* to produce an effect" (emphasis in original).

Let us explain. First, we remain convinced that power is operative even when *A unconsciously* exercises it or when he is aware of exercising it and produces unintended effects. To argue differently, as Debnam does, is to severely restrict the study of power in a societal context. As students of power, to quote from our essay, "our main concern is not whether the defenders of the status quo use their power consciously, but rather if and how they exercise it and what effect it has on the political process and other actors within the system" (*Power and Poverty*, p. 50). Dennis Wrong nicely illustrates the relationship between power and unintended effects: "It is only because a mother exercises socially approved power over her children that she may unintentionally shape their personalities along lines that are repugnant to her and defeat her most cherished hopes."[10]

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[6] Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality* (New York: Harper & Row, 1972).

[7] *Ibid.*, pp. 272–285.

[8] *Ibid.*, p. 335. Emphasis in original.

[9] *Ibid.*, p. 342.

Second, we recognize the existence of *indirect* power relationships, i.e., those in which communication between the power wielder and the power receiver is wholly or partially obstructed. This kind of situation is well exemplified by situations in which power is successfully employed to suppress a prospective challenge to the status quo. In fact, it often happens that neither *A* nor *B* is aware of the other's existence, yet one can exercise power over the other. Contrary to our earlier belief, then, "clear communication" between them is not a necessary condition of a power relationship. It is necessary that *B* be aware that, contrary to his wishes, he is unable to elevate his grievance to the status of public issue without violating "the rules"; and that the rules, which are created or reinforced by an exercise of power, are a barrier against his conversion of the grievance into an issue. If he chooses, however, to resist the rules and thereby invite invocation of the sanction (e.g., strong social disapproval or physical punishment), the attempt to exercise power has failed. The manner in which *A* exercises power over *B* in this kind of situation should no more be discounted because of its indirect character than should the exercise of power by a general against his foot soldiers. In the former case, power is brought to bear through prevailing norms and institutions; in the latter, through a chain of command.

Third, in our essay we proposed a causal view of power, asserting that "*to the extent* that a person or group—consciously or unconsciously—creates or reinforces barriers to the public airing of policy conflicts, that person or group has power" (*Power and Poverty*, p. 8). This formulation is excessively stringent in implying that *A* exercises power only when it is a necessary and sufficient condition of gaining *B*'s compliance. If the necessary-and-sufficient test were held to, there would be few instances, individual or social, in which it could be shown that power was exercised. Especially would this be true when power is exerted in order to shape or reinforce a mobilization of bias as a shield against a potential issue that threatens to become actualized. Here the relationship is indirect and thus the causal connection between *A*'s action and *B*'s response is usually loose. A more useful formulation of power is one which states that a person or group exercises power to the extent that he, she or it *contributes* to shaping or strengthening barriers to public airing of grievances.[11] The problem confronting the analyst is judging the extent of the contribution, a difficult task and one that certainly does not invite exactitude.

## Conclusion

In summary, our approach to power overlaps in important respects that of Debnam and others whose views he represents. Most notably, we accept with few reservations their generalizations about and mode of inquiry into the decision-making face of power. But let no one mistake the extent of difference between us and them.

The essence of the difference can be stated simply: For us, but not Debnam and his cohorts, the test of a construct's worth is the contribution it makes toward gaining a full understanding of the nature and uses of power in a polity. Empirical verification is much to be desired but it is not foremost in importance. What *is* foremost is a solid conceptual framework. Toward that we are building, the dogmatic empiricists to the contrary notwithstanding.

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[10] Dennis H. Wrong, "Some Problems in Defining Social Power," *American Journal of Sociology*, 73 (May, 1968), 673–681, at 677.

[11] For a defense of this position, see Petro Georgiou, "The Concept of Power: A Critique and an Alternative" (Unpublished; University of Melbourne, 1974).

# Rejoinder to "Comment" by Peter Bachrach and Morton S. Baratz

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Bachrach and Baratz argue that a decision-making approach carries certain necessary assumptions concerning the nature of the political process, and that these assumptions cause research to be focused on a limited area of the political process. They take this view, I believe, because they have confused a decision-making approach with a pluralist approach. It is interesting to note that what they have to say on the basic assumptions of "the decision-making model" represents the argument raised by them in their first article "The Two Faces of Power," but which was there directed against the *pluralist* model. Only if one is prepared to believe that the pluralist interpretation of decision-making analysis is alone valid can one then indulge in the sleight of hand necessary to condemn *any* decision-making approach for "pluralist sins."

Any method of study may legitimately be modified to suit the purposes of the researcher. Snyder for example suggested that decision-making analysis should focus on decisions "made *within* the governmental structure,"[1] but this proposal was dependent on a restricted view of the nature of authority which suited Snyder's preoccupation with foreign policy making. Dahl's research design for his New Haven study excluded a wide variety of decisions on the reasonable grounds of economy, and his choice was rationalized on pluralist premises. In neither case did the choice of focus spring from the theoretical imperatives of a decision-making approach. The researcher had to import other criteria to sort his way through the heap.

So whether a decision is made within the governmental structure or outside it; whether it is a decision to act or not to act, to decide or not to decide, covert or overt; whether it is made by the hero or the villain; whether it is readily identifiable in formal terms, or whether it is assumed from the observation of its consequences, the short answer is that it may be encompassed within a decision-making approach. There is no reason that anyone making this assumption should have any more difficulty in recognizing and analyzing the forms, processes, and instances of status-quo dominance than Bachrach and Baratz themselves. After all, as they point out, "a nondecision, as we define it, is a decision." And, lest the point should go unnoticed, a decision is a decision, is a decision! A decision-making approach is adequate to deal with the analysis of decisions, whatever alternative name they are given.

Bachrach and Baratz have made some specific charges of inaccuracy in my analysis. They claim, for example, that I am wrong to suggest that what they describe as nondecision making can be said to have two forms. In refuting this suggestion they quote their own statement that "the primary method for sustaining a given mobilization of bias is nondecision-making." The implication from this isolated sentence is clearly that mobilization of bias cannot therefore itself be a form of nondecision making. If Bachrach and Baratz were consistent, this would be all that needed to be said. But since a nondecision "is a decision that results in suppression or thwarting of a latent or manifest challenge to the values or interests of the decision maker," then clearly any agent effecting such a decision *must* be a nondecision maker. Can the mobilization of bias be held to produce such effects? Bachrach and Baratz can certainly be cited in support of such a view. Their reference to the *unconscious* creation or reinforcement of barriers to the public airing of conflicts can be interpreted in no other way, and neither can the statement in their comment that "mobilization of bias . . . actually regulates the nature and extent of conflict."

Whether the phrase "covert control" is regarded as an accurate description of the other form of nondecision making depends on whether one can envisage a situation where a status-quo defender resorts to the exploitation of "prevailing norms, precedents, myths, rituals, institutions and procedures" and where he is explicit about his objectives. The assumption behind my description is that the overwhelming majority of occasions will involve a decision whose true intention is not made explicit to the "victims." Bachrach and Baratz's objection, then, seems to be based on a misunderstanding of my use of the term "covert." I mean that the *purpose* is hidden, not necessarily the *action which embodies the purpose*.

The third section of their "Comment," referring to my discussion of their definitions of power and its correlates, is disingenuous. They certainly do not contest the substance of my argument, except in one small instance to which I refer below. Instead they suggest that in some further unstated way, which by implication invalidates my critique, I have gone beyond the purpose of their discussion. It is as well to bear in mind that this purpose "was simply to distinguish rigorously among a group of concepts that are generally regarded as synonymous, *in the interest of a greater degree of analytical and expository precision*" (emphasis added). The object of my critique is to show that they have failed to do this. If they disagree with my argument, they should explain the grounds of their disagreement and not fall back on the suggestion that I have taken them too seriously.

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[1] Richard C. Snyder, "A Decision-Making Approach to the Study of Political Power," in *Approaches to the Study of Politics*, ed. Roland Young (Evanston, Illinois: Northwestern University Press, 1958), p. 16.

The only grounds on which they attack my critique is that I have misunderstood their references to sanctions. Should anyone care to read through the relevant sections, they will see that Bachrach and Baratz state that there are "no severe sanctions" in the case of influence and authority, and that I take them to mean that there *are* sanctions of some sort, but that they are not severe. Why should they qualify what they mean to be absolute? But since I argue that "sanction" is a worthless means of distinguishing between power and its correlates, surely their response is misdirected.

A similar level of confusion is reflected in their reference to my approach to power which they claim is open to the same criticism that I level against their formulation—that it has "more relevance to a description of an individual state than a political relationship." In attempting to substantiate this they cite my emphasis on "*the intention* to produce an effect" yet ignore the further reference three sentences later to "the intentions of actors in the community, *institutional* as well as individual" (emphasis added). I cannot imagine it necessary to explain the significance of my reference to "institutional" in this context.

There are other points where Bachrach and Baratz make claims on my behalf to which I do not subscribe, but an enumeration of minor grievances becomes tedious. I would prefer to move on to their comment that I have presented no guide as to how I would structure an alternative approach—which was, of course, peripheral to my main concern of demonstrating the inconsistencies and inadequacy of their work. I suggested, though, that the study of power would have to be couched in terms which embrace both intentions and effects. Since this would not be sufficiently specific for a focused inquiry I noted that the observer must attack the problem "from a perspective he determines himself, or which emerges from the community he chooses to study." Similarly, Bachrach and Baratz have suggested that any discussion of power should embrace both decision making and what they choose to call nondecision making. They nevertheless have to provide a focus for their empirical study by referring to criteria which are extrinsic to those concepts. "An empirical analysis of the relationship between the anti-poverty effort and the political process in Baltimore, Maryland"[2] is, surely, both (a) an exercise which they thought would be favorable to a demonstration of their views, and (b) a topic which they think is important. This is perfectly legitimate. There is no reason why they should deplore in my case a procedure which they have followed in their own—unless they feel that they have done more.

I agree with them, though, that the words power, authority, coercion, influence and manipulation have different meanings. My intuitive sense of the ordering of these concepts is as follows. Power is treated at sufficient length in my article. It is the generic term and the one which is used to denote an outcome but which cannot therefore be used to explain the causes of that event. Authority and coercion are the bases of power. In the extreme cases they can themselves account for the production of an intended effect. I may accede to a request because I regard it as unquestionably authoritative (that I recognize a duly elected leader, or accept the advice of an expert), or because it is so coercive that I have no choice but to submit to what I regard as an alien interpretation of my situation. But situations are rarely so straightforward. Where there are no clear and compelling imperatives there is scope for influence. And the language of influence will be more or less authoritative or coercive. Influence, then, is to be distinguished from authority and coercion in that it is not a power base but is the way we describe the exploitation of such a base. Manipulation is a closely related term. It refers, however, to the covert use of a power base.

This simpler formulation emphasizes that coercion is, with authority, an equally significant theoretical aspect of power, rather than being merely one amongst a variety of concepts. Power is frequently used as a term denoting the possession of a faculty which is neither authoritative nor coercive. Authority and coercion, between them, though, describe the limits of legitimate and nonlegitimate action. This states briefly what is contentious and would need elaboration. But where these terms are removed from the center of political analysis and replaced by power masquerading as a term of technical virtuosity, it is easy to overlook the problems of public morality that Bachrach and Baratz are so properly concerned with.

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[2] Peter Bachrach and Morton S. Baratz, *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970), p. viii.

In view of the length of their "Comment," and of the period of time since their ideas were first mooted (twelve years), it is significant that they have been unable to attempt a more coherent justification of their position. That such is necessary should be apparent from their lament that "yet another competent scholar" has failed to appreciate their argument. They try to resolve the problem too easily in my case by attributing it to my "slovenly reading and analysis" as they describe it. But it may well be time for them to consider the possibility that there are serious difficulties in their theoretical work. Before they attempt to build the "solid conceptual framework" that they promise, they should inspect the foundations more closely.

# On the Apparent Paradox of Participation: A New Proposal

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In his *Economic Theory of Democracy*, Anthony Downs reached the conclusion that voting is usually an irrational act.[1] Given that voting is costly in time and effort involved in getting to the polls and making a decision and given that the probability any one voter can affect the outcome diminishes very rapidly as the number of voters increases, it appears that a rational, utility-maximizing citizen will most probably abstain. Only if the citizen perceives a phenomenally large difference between (or among) the candidates or if the number of voters is small will he go to the polls.

Empirically, however, we know that substantially more than half of all eligible voters go to the polls in American presidential elections where the probability of any one voter's affecting the outcome is smaller than in any other election. If Downs is correct, at least half of the American electorate is irrational. But of what validity is a rational choice theory which characterizes most individuals as irrational? For this reason, theorists have attempted to modify Downs's theory so as to allow it to better explain why some rational citizens vote while others abstain.

Some of these revisions, like that presented by Riker and Ordeshook, have simply added variables to the original Downs theory so as to make voting rational for some substantial group of citizens.[2] Others, like Ferejohn and Fiorina, have substituted a different calculus of rationality for Downs's expected utility model.[3] Under close examination, however, each of these revisions appears to involve theoretical and empirical problems of the same order of magnitude as Downs's original theory. I discuss these theories and their related problems in the next section. Following this I present a new though seemingly obvious modification of the Downs theory, which will, I believe, substantially improve the empirical validity of his theory while avoiding the problems associated with the other revisions.

## Previous Theories

Using the notation of Riker and Ordeshook, Downs's original theory can be represented by the following equation:

$$R = PB - C \tag{1}$$

where $R$ is the satisfaction, in utiles, a voter will receive from voting, $B$ is the difference in utility, in utiles, that a voter perceives between his most and least preferred candidate, $P$ is the probability that a voter will himself bring about benefit $B$ by voting, and $C$ is the cost, in utiles, of voting.[4] In the Downs model, if $R_i > 0$, citizen $i$ votes; if $R_i \leq 0$, he abstains.[5] Assuming $C_i > 0$, the only way for $R_i$ to be greater than zero is for $PB > C$. But as the number of voters increases, $P$ declines rapidly so that for any actual election, $P$ is likely to be very small. In turn this implies that $B$ must be very large or else $PB \leq C$ and the citizen abstains. As one example, Tullock has shown that where $C$ equals one utile and $P$ equals .000,000,1, even a $B$ of 10,000 utiles is insufficient to cause a rational citizen to vote (in fact, in this case $B$ must be larger than 10,000,000 in order to make $PB \geq C$).[6]

The major solution to this problem suggested by Riker and Ordeshook is to add a variable $D$ to the original Downs equation.[7] This new variable is equal to the utility a citizen receives from the act of voting, regardless of who wins the election. That is, it is the "satisfaction [derived] from compliance with the ethic of voting," from "affirming allegiance to the political system," from "affirming a partisan preference," etc.[8] From its definition it is assumed $D > 0$ and that:

$$R = PB - C + D. \tag{2}$$

Thus, what Riker and Ordeshook have done is to insure that $R$ can be positive for a substantial set

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[1] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), p. 274.

[2] William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25–42. See also William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), chap. 3.

[3] John A Ferejohn and Morris P. Fiorina, "The Paradox of Not Voting: A Decision-Theoretic Analysis," *American Political Science Review*, 68 (June, 1974), 525–536.

[4] Riker and Ordeshook, "Calculus," p. 25.

[5] Technically the condition $R_i > 0$ is a necessary but not a sufficient condition. For example, if for individual $i$, $R_i$ is positive he still may not vote if some alternative activity to voting gave him greater utility. $R_i > 0$ is sufficient *only* when it is assumed that the maximum utility of all alternatives to voting is 0. Inasmuch as $R_i > 0$ is a nice shorthand notation such an assumption is made in this paper.

[6] Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: The University of Michigan Press, 1967), p. 110.

[7] Riker and Ordeshook, "Calculus," pp. 27–28.

[8] *Ibid.*, p. 28.

**Table 1. Empirical Test of the Riker-Ordeshook Calculus of Voting**[9]

| | "Citizen Duty" Score | | | | | |
|---|---|---|---|---|---|---|
| | High | | Medium | | Low | |
| Expected closeness of election result | High P.D. | Low P.D. | High P.D. | Low P.D. | High P.D. | Low P.D. |
| Close | 91% | 83% | 85% | 71% | 63% | 44% |
| Not Close | 86% | 74% | 77% | 71% | 62% | 39% |

(P.D. = party differential; i.e., a measure of B.)

of citizens even if they can have no influence on the outcome of an election.

Is this a satisfactory solution? To answer this question one must distinguish between the empirical merits of their solution (does it allow us to better explain voting) and the theoretical merits (how well does it fit in with a rational choice model). Having made this distinction, one can say that empirically the Riker and Ordeshook revisions have merit, while theoretically their revisions are problematical.

Consider first the empirical merits. Table 1 is a reduced version of the complex Table 3 of Riker and Ordeshook. The first thing to note in this table is that the original Downs theory has some empirical validity. When voters expect an election to be close (i.e., when $P$ is relatively large) as well as when there is a high $B$ (i.e., the voter perceives a large difference between the candidates), more citizens vote. But it is clear in the table that these are minor effects compared to the effect of the $D$-term as measured by "Citizen Duty" scores. As $D$ declines, the per cent voting declines much more rapidly than when $B$ declines or $P$ increases. Hence, the Riker-Ordeshook revision adds considerable explanatory power to the original Downs model.

Theoretically, however, the Riker-Ordeshook theory creates a major problem. As Barry notes:

> Riker [and Ordeshook say] . . . that people vote because they derive satisfaction from voting for reasons entirely divorced from the hope that it will bring the desired results. This may well be true but it does not leave any scope for an economic model to come between the premises and the phenomenon to be explained. Instead, the question shifts back to: 'Why do some people have this kind of motivation more strongly than others?'[10]

In other words, knowing that the $PB$ term of the Downsian equation is likely to be quite small, the Riker-Ordeshook revisions essentially reduce the Downs theory to:

$$(3) \qquad R = D - C.$$

Given that both $C$ and $D$ (as defined by Riker and Ordeshook) are likely to be constants, it can be seen that in the Riker-Ordeshook model, the decision to vote or not to vote is largely independent of the particulars of specific elections.

Ferejohn and Fiorina attempt to improve upon this state of the theory by scrapping the expected utility model and substituting in its place Savage's minimax regret criterion.[11] Under this criterion, a citizen "chooses the act which minimizes his maximum regret."[12] This is illustrated in Table 2, identical to Table 3 of Ferejohn and Fiorina.[13] In constructing this table Ferejohn and Fiorina standardized the voters utility function so that the utility of candidate 1 (the most preferred candidate) winning is 1, and the utility of candidate 2 winning is 0.
The symbols used in this table have the following ing meaning:

$S_1$: Candidate 1 (the most preferred candidate) wins by more than one vote without the citizen's vote
$S_2$: Candidate 1 wins by exactly one vote without the citizen's vote
$S_3$: Candidate 1 and candidate 2 tie: (a coin flip decides winner) without the citizen's vote
$S_4$: Candidate 2 wins by exactly one vote without the citizen's vote
$S_5$: Candidate 2 wins by more than one vote without the citizen's vote
$V_1$: The citizen votes for candidate 1
$V_2$: The citizen votes for candidate 2
$A$: The citizen abstains

In this table the maximum "regrets" for each row are $C$, 1, and $1/2$-$C$ respectively. From the

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[9] This table is taken from Brian Barry, *Sociologists, Economists, and Democracy, Themes and Issues in Modern Sociology* (London: Collier-Macmillan, 1970), p. 17.

[10] *Ibid.*, p. 16.

[11] Ferejohn and Fiorina, pp. 527–528.

[12] *Ibid.*, p. 528.

[13] *Ibid.*

**Table 2. Regret Matrix for Two-Candidate Election**

| Acts | $S_1$ | $S_2$ | $S_3$ | $S_4$ | $S_5$ |
|---|---|---|---|---|---|
| $V_1$ | $c$ | $c$ | 0 | 0 | $c$ |
| $V_2$ | $c$ | $1/2+c$ | 1 | 1/2 | $c$ |
| $A$ | 0 | 0 | $1/2$-$c$ | $1/2$-$c$ | 0 |

Assumption: $1/2>c$.

$c<1/2$ assumption, $V_2$, voting for the least preferred candidate is not viable because $c<1/2<1$ and hence, strategy $V_1$ dominates $V_2$. Similarly, strategy $A$ dominates $V_2$. Thus, in a two-candidate contest, a citizen either votes for his most preferred candidate or abstains. But a distinction can also be made between these two; if the maximum regret of $V_1$ is less than the maximum regret of $A$, the citizen will elect strategy $V_1$. That is $c<1/2-C$, which implies that he will vote for candidate 1 if $c<1/4$. Substantively, he will vote if his costs of voting are less than one-fourth of the difference between his most-preferred and least-preferred candidate.

To see that this is not equivalent to the expected utility solution of Downs, consider the previous example where a citizen would gain 10,000 units of utility if his most-preferred candidate won the election and zero if his most-preferred candidate lost. Remember that it cost him 1 utile to vote and that the probability he would decide the election was .000,000,1. Under an expected utility model this citizen would not vote. But he would vote if he were a minimizer of maximum regret for $10{,}000-0>.25=1/4C$. Hence the Ferejohn–Fiorina model predicts a higher level of voting than does the Downs model.

It is thus clear how Ferejohn and Fiorina "solved" the paradox of participation; they found a means of eliminating $P$, the probability that a citizen, by voting, would affect the outcome. In their model, $P(S_3)$, the probability the election results in a tie, and $P(S_1)$, the probability candidate 1 wins by more than one vote, as well as the probabilities for all the other states of nature are equal.[14] This is clearly unrealistic. Empirically it is almost always true that $P(S_3)<P(S_1)$; in fact, $P(S_3)$ is likely to be a very small number as are $P(S_2)$ and $P(S_4)$. If the maximum regret for any of $V_1$, $V_2$, or $A$ (see Table 2) is in one of these columns, is it not rational for the citizen to discount the possibility of himself experiencing the regret? For example, the maximum regret for abstaining is $1/2-C$ if either $S_3$ or $S_4$ are the states of nature; if the probability of either $S_3$ or $S_4$ occurring is very small (e.g., $1\times10^{-8}$) should not the citizen consider his maximum regret for abstaining to be 0 as it would be if $S_1$, $S_2$, or $S_5$ were the state of nature? Analogously, consider a citizen deciding whether to play the following game: a number from 1 to 1000 is to be drawn from a hat containing each of the numbers from 1 to 1000 only once (i.e., the hat has only 1000 numbers in it); if number 500 is drawn the citizen loses \$1.00, if any other number is chosen he wins \$10.00. His maximum regret from playing is \$1.00, from not playing, 0. The minimum of these is 0 *so he would not play*! This is clearly not an intuitive nor, I suspect, empirically valid conclusion.

**Table 3. Relevant Electoral Outcomes for a Two-Candidate Election**

| States of Nature | Description |
|---|---|
| $O_1$ | Candidate 1, the most preferred candidate wins by more than one vote. |
| $O_2$ | Candidate 1 wins by exactly one vote. |
| $O_3$ | Candidate 1 and Candidate 2 tie; a coin flip decides winner. |
| $O_4$ | Candidate 1 loses by exactly one vote. |
| $O_5$ | Candidate 1 loses by more than one vote. |

Moreover, an examination of Table 1 will show that the minimax regret principle cannot account for some of the variance in voting. In all categories, there was a higher or equal percentage of voting when the election was perceived to be close than when the election was not perceived to be close. This difference, though admittedly small, cannot be explained by the regret model. Hence, use of this model involves the violation of intuition as well as a reduction in explanatory power.

## A New Reformulation

In the Downs expected utility model, $R$, the expected utility of voting equals $PB-C$ where $B$, the "party differential" equals $B_1-B_2$, the difference in utility derived from the victory of the most-preferred candidate and the utility derived

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[14] Very technically this is untrue because the Ferejohn-Fiorina model has no probabilities in it whatsoever. Their model without probabilities, however, is equivalent to an equiprobable model.

**Table 4. Hypothesized Utility Associated with States of Nature when Voting and Abstaining**

| Symbol | Conclusion | Rationale |
|---|---|---|
| $U_{1j}^v$ | Positive, moderate size. | The utility citizen *j* would receive from the victory of his most preferred candidate as well as from contributing to the size of the victory. Likely to diminish as the margin of victory increases. |
| $U_{2j}^v$ | Positive, large. | The utility *j* would receive from deciding the election in favor of his most preferred candidate. |
| $U_{3j}^v$ | Positive, large but less than $U_{2j}^v$. | The utility *j* would receive from saving his most preferred candidate from sure defeat. |
| $U_{4j}^v$ | Negative, moderate. | The disutility *j* would receive from the loss of his most preferred candidate. |
| $U_{5j}^v$ | Negative, moderate. | Same as for $U_{4j}^v$. |
| $U_{1j}^a$ | Positive, moderate. | The utility *j* would receive from the victory of his most preferred candidate. |
| $U_{2j}^a$ | Positive, small. | Same as for $U_{1j}^a$. |
| $U_{3j}^a$ | Negative, large. | If *j* had voted he could have decided the election. But he did not and now his most preferred candidate has only a .5 probability of winning. This assumes a "Damn, if I had only voted" psychology, especially if his candidate loses the flip. |
| $U_{4j}^a$ | Negative, large. | Almost the same as above; here if *j* had voted he could have given his most preferred candidate a .5 probability of winning whereas, by abstaining, his most preferred candidate loses. |
| $U_{5j}^a$ | Negative, moderate. | The disutility *j* would receive from the loss of his most preferred candidate. |

from the victory of the least-preferred candidate. Thus, in this model the *B* term represents only the utility difference derived from the victory of one candidate over the other. I will attempt in the following to show that this is a quite narrow interpretation of *B* and that, in particular, it fails to take account of how the victory is attained.

The five relevant electoral outcomes for a two-candidate election are given in Table 3. Let $U_{ij}^v$ be the utility that citizen *j* perceives he would derive if outcome $O_i$ occurs when he votes and $U_{ij}^a$ be the utility when $O_i$ occurs when he abstains. I interpret $U_{ij}^v$ and $U_{ij}^a$ as follows: let citizen *j* be deciding whether or not he will vote; he asks himself, "How would I feel if I voted and $O_i$ were the outcome?" This is $U_{ij}^v$. Similarly, he asks, "How would I feel if I did not vote and $O_i$ were the outcome?" This is $U_{ij}^a$. Hypotheses about the magnitude of these outcome related utilities are presented in Table 4.

The main feature of these hypothesized magnitudes is that they are not symmetrical for voting and abstaining.[15] Most important, I hypothesize that the utility derived from deciding an election in favor of a voter's most-preferred candidate, $U_{2j}^v$, is positive and is large, while the utilities of failing to vote and, then having one's most-preferred candidate lose ($U_{4j}^a$) or draw ($U_{3j}^a$) are negative (i.e., are disutilities) and are large. In part these outcome-related utilities reflect the utility a voter would receive if his most-preferred candidate won or lost. But they also reflect the utilities (or disutilities) a citizen receives from deciding (or failing to decide) the electoral outcome. For example, if a citizen votes and $O_2$ occurs, he will receive utility from the victory of his most-preferred candidate as well as from having decided the election himself. I would venture that it is reasonable to assume this latter degree of utility to be quite large. And, analogously, it appears reasonable to assume great disutility from failing to decide an election when it was possible to do so.

To this point the model is quite similar to the minimax regret model. The similarity ends, however, with the introduction of probabilities. Let $P_{ij}$ be citizen *j*'s subjective estimation that outcome $O_i$ will occur when he votes and let $Q_{ij}$ be $j$'s estimation that $O_i$ will occur when he does not vote. I require that

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[15] A somewhat similar situation is the choice between gambling (seeking risk) and insurance (avoiding risk); see Milton Friedman and Leonard J. Savage, "The Utility Analysis of Choices Involving Risk," *Journal of Political Economy*, 56 (August, 1948), 279–304.

$$\sum_{i=1}^{5} P_{ij} = \sum_{i-1}^{5} Q_{ij} = 1.$$

This requirement for the probabilities together with an assumption that the electorate is relatively large determine that $P_{1j}$, $P_{5j}$, $Q_{1j}$, and $Q_{5j}$ are relatively large while $P_{2j}$, $P_{3j}$, $P_{4j}$, $Q_{2j}$, $Q_{3j}$, and $Q_{4j}$ are quite small. This is equivalent to saying that the probability of any one citizen's affecting the electoral outcome is quite small whether he votes or not. These latter probabilities increase rapidly, however, as an election is perceived to be a close contest.

With these specifications I can state the citizen's expected utility from voting as follows:

$$\text{(4)} \qquad \sum_{i=1}^{5} P_{ij}(U_{ij}^{v}) - C,$$

where, as in the Downs model, $C$ is the utility cost of voting. Similarly, the expected utility from abstaining is

$$\text{(5)} \qquad \sum_{i=1}^{5} Q_{ij}(U_{ij}^{a}).$$

Hence, in this model, it is rational for citizen $j$ to vote if

$$\text{(6)} \qquad \sum_{i=1}^{5} P_{ij}(U_{ij}^{v}) - C > \sum_{i=1}^{5} Q_{ij}(U_{ij}^{a}).$$

Transposing terms as follows, if the difference between a citizen's expected value of voting and of abstaining exceed the costs of voting, he will vote:

$$\text{(7)} \qquad \sum_{i=1}^{5} P_{ij}(U_{ij}^{v}) - \sum_{i=1}^{5} Q_{ij}(U_{ij}^{a}) > C.$$

Examining Table 4, we can see that $U_{4j}^{v}$, $U_{5j}^{v}$, $U_{3j}^{a}$, $U_{4j}^{a}$, and $U_{5j}^{a}$ are negative. Furthermore, equation (7) can be written as:

$$\begin{aligned}\text{(8)} \quad & P_{1j}(U_{1j}^{v}) + P_{2j}(U_{2j}^{v}) + P_{3j}(U_{3j}^{v}) \\ & + Q_{3j}(U_{3j}^{a}) + Q_{4j}(U_{4j}^{a}) + Q_{5j}(U_{5j}^{a}) \\ & > C - P_{4j}(U_{4j}^{v}) - P_{5j}(U_{5j}^{v}) \\ & + Q_{1j}(U_{1j}^{a}) + Q_{2j}(U_{2j}^{a}).\end{aligned}$$

Now all the factors on the left of the inequality contribute positively to voting while all the factors on the right side contribute positively to abstention. What may initially seem surprising is that certain of the utilities connected with abstention may be important inducements to vote while certain of the utilities associated with voting contribute to abstention. Said another way though, this is less surprising; that is, the disutility of abstaining contributes to voting while the disutility of voting contributes to abstaining.

Does this model predict more voting than the Downs model? I believe that it does if the hypotheses in Table 4 are correct for a substantial number of citizens. This can be shown by simplifying (8) with the help of two assumptions. For a reasonably large electorate, $P_{ij}$ will closely approximate $Q_{ij}$. If then $U_{1j}^{v}$ is assumed to equal $U_{1j}^{a}$ (utility derived from a victory by the more preferred candidate is the same whether $j$ votes or abstains) and if $U_{5j}^{v}$ and $U_{5j}^{a}$ are equal (the disutility of a loss by more than one vote of the most preferred candidate is the same whether $j$ votes or abstains), then (8) reduces to:

$$\begin{aligned}\text{(9)} \quad & P_{2j}(U_{2j}^{v}) + P_{3j}(U_{3j}^{v}) + Q_{3j}(U_{3j}^{a}) \\ & + Q_{4j}(U_{4j}^{a}) > C + P_{4j}(U_{4j}^{v}) \\ & + Q_{2j}(U_{2j}^{a}).\end{aligned}$$

Because, from Table 4, $U_{4j}^{v}$ is only moderately large, while $P_{4j}$ is likely to be quite small, $P_{4j}(U_{4j}^{v})$ is likely to be very small. The same is true for the same reasons for $Q_{2j}(U_{2j}^{v})$. Hence, the only significant term to the right of the inequality is $C$, the cost of voting. But each of the $U_{ij}$ terms on the left of the inequality in (9) is assumed to be large. Hence, even though all the corresponding $P_{ij}$ and $Q_{ij}$ terms are quite small, the terms on the left in (9) may not disappear, so that added together, they may well be larger than $C$ for a substantial number of citizens.

Further, note that the model predicts increased voting in close elections because, in this case, $P_{ij}$ and $Q_{ij}$ for $i = 2, 3, 4$, are likely to be larger than in elections perceived to be one sided. And, as these $P_{ij}$ and $Q_{ij}$ factors increase, the magnitude of the factors on the left side of the inequality in (9) increase while $C$ on the right side remains constant.

### Summary and Conclusions

It should be clear that the theory presented here departs from both the expected utility model of voting and the Ferejohn-Fiorina minimax regret model. As noted above, it completely changes and broadens the interpretation of the Downsian

*B* term.[16] This is accomplished by deleting the Downsian assumption that utility is derived only when one affects the outcome by voting and, in its place, putting the assumption that an individual associates different utility payoffs with each of a set of electoral outcomes, these outcomes being jointly determined by the state of nature and the individual's actions. Intuitively this seems a more appropriate assumption than either the restricted Downsian assumption or the Ferejohn-Fiorina assumption that citizens make an equiprobable assumption over the set of relevant electoral outcomes. Further, with some assumptions about the magnitude of the set of outcome related utilities, it appears that this model leads to predictions of more voting than does the original Downs model without, at the same time, introducing a different rationality calculus or a problematic *D*-variable.

[16] One different reinterpretation of the *B* term is presented in George J. Stigler, "Economic Competition and Political Competition," *Public Choice,* 13 (Fall, 1972), 91–106.

# The Paradox of Not Voting: Comment

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It is continually demonstrated in the pages of the *Review* that formal theorists in political science have great difficulty in communicating with one another, in reaching consensus, e.g., on the rationality of minimal winning coalitions and the effects of vote-trading. It has always puzzled me how such confusion and controversy could surround simple arguments which are—or at least appear to be—mathematical. Reading the extraordinarily clear contribution of Ferejohn and Fiorina ("The Paradox of Not Voting: A Decision Theoretic Analysis," *APSR* 68 [June, 1974], 525–536), I am led to the conjecture that a sort of natural selection is involved, that the more lucid papers are so patently silly that many of them fail to achieve publication, leaving the field to papers in which the nonsense is at least obscure.

The authors ask whether "the principal explanation of the voting act is found in its investment or its consumption aspect," and I take their contribution to be the suggestion that some voters are rational investors who obey not the principle of maximization of expected utility, but the more esoteric principle of "minimax-regret." (We cannot pause to consider whether the voting act, any more than the sex act or any other darkly functional social behavior suffused with myth and ritual, is adequately treated with economic categories.) The idea of minimax-regret is that the decision-maker pays no heed to the *probability* that nature is in particular states, but rather democratically scans all *possible* states of nature, saying to himself, "Suppose nature is this way—if I do $V^1$, my regret will be $c$; if I do $A$, my regret will be 0 . . .", until he has constructed for himself a "regret matrix," states-of-nature by courses-of-action. (For the precise meaning of "regret," see below.) Examining this matrix, the decision maker chooses that action which exhibits the smallest maximum regret. The authors suggest that someone making a voting decision in a straight fight will ordinarily associate a regret of $c$—the cost of bothering to go to the polls—with a vote for either candidate, and a regret of 0 with abstaining: the idea is that ordinarily one candidate will win by a number of votes, and the voter will realize after the election that his vote did not matter; he will regret that he bothered. But, ah, it is just *possible* that the voter could make or break a tie; if he abstains and his candidate loses by one vote, he will be remorseful indeed. Thus, obeying the principle of minimax regret, he votes.

Let us consider for a moment another remote possibility. There are perhaps 50,000 Americans killed annually in motor-vehicle accidents; say 150 a day—it has long been a commonplace that deaths are *not* highly concentrated in holiday-weekend "carnage." For every death, there are several serious injuries; say serious casualties amount to 750 a day. Suppose that on a presidential election day, *one per cent* of these casualties are suffered in traveling to and from the polls; thus the voting act will be profoundly costly for about one voter in ten million. The probability of a tied presidential election is more difficult to assess, owing in part to the electoral college, in part to the possibility of litigation of a very close result, and so on. Let us suppose a popular election, and that a "close" election is one in which the probability distribution of votes for the Democratic candidate is rectangular with a range of one million; in such a situation, the chance that one's vote would be tie-making or tie-breaking is on the order of one in a million.

If a minimax-regretter is going to dip down six factors of ten in probability to pick up the possibility that his sole vote may decide a presidential election, it is difficult for me to see that he will not dip down another factor of ten and pick up the possibility that he will be run over; I am sure there are many professors of political science who would gladly give their legs to elect a Democratic president, but I do not believe such intensity of sentiment is widely distributed in the population. Note that numerical probabilities are actually irrelevant to a minimax-regretter—*it is enough to recognize the possibility*. We may consider it fortunate that few voters will have imaginations vivid enough to consider such outcomes as: (a) one elects one's candidate by a single vote, (b) one then is in an accident entailing total paralysis, and finally, (c) languishing in the nursing home, one sees one's candidate metamorphose into another Caligula.

This decision-theoretic *grand guignol* is intended to suggest that the juridical outcome of an election—win, lose, or draw,—is not the only uncertainty facing a potential voter. Ferejohn and Fiorina suppose that the voter's cost of voting and the utility he associates with the various outcomes are known and fixed parameters; but surely most voters so thorough as to consider the possibility of a tied outcome will also consider the possibility that their costs and utilities are to some degree uncertain as well. For the decision theorist, alternate states of nature are the symbols, $S_1$, $S_2$, $S_3$, printed upon the page; for the man in the world, the alternate states of nature frequently must seem indefinite in character and infinite in number, and any final decision based on minimax-regret (which assumes strict symmetry among possible states of nature) will depend mostly on how imaginative the decision maker is in formulating the alternate states of nature.

In a straight fight, there may be some people who choose to vote in just the way the authors suggest; I doubt they are very numerous, and I expect that it would in practice be exquisitely difficult to locate them, for it would be necessary to distinguish them from other persons for whom tied outcomes would be salient for other reasons: some of these would be utility maximizers who grossly exaggerate the probability of a tied outcome (to the extreme of those who apply the principle of insufficient reason, supposing tied and nontied outcomes equiprobable!), and others would associate *different* utilities with the victory of a particular candidate, depending on whether their vote was *necessary* for his victory. This latter possibility is quite distinct from "regret" in the technical sense, a point which wholly escaped the authors and presumably most of the eight theorists whom they thank in their first footnote. Regret has nothing to do with guilt or responsibility—it is the difference between what you "get" acting the way you did and what you might have gotten if you had known in advance the true state of nature and acted optimally on that knowledge (e.g., not voted for a candidate who was going to win anyway). "What you get" is the utility you associate with the victory of a particular candidate (independent of how he is elected—by a landslide, a single vote, or a coin-flip), decremented—if you vote—by your cost of voting. In spite of this, the authors attribute to a minimax-regretter the reasoning, "My God, what if I didn't vote and my preferred candidate lost by one vote? I'd feel like killing myself." Now one does not normally feel suicidal if one's preferred candidate *loses*; one might, however, if one were *responsible* for his loss. A minimax-regret voter actually feels slightly better if his candidate loses by one vote with the voter abstaining (the payoff is zero), than he feels if the candidate loses in spite of the voter's support: (the payoff is $-c$, zero decremented by the cost of voting). The reader who finds this bizarre should examine cells $(S_4, A)$ and $S_5V_1$) of the authors' Table 2, page 527. This is not the "regret matrix" but the "payoff matrix" —it is the latter that tells you how the voter "feels" after the outcome. (I hope the authors will not dare to argue that "payoffs" and "regrets" in their model are somehow empirically independent experiences: the regrets are nothing more than arithmetic differences of payoffs, and cannot be interpreted as an experience of remorse or pride separate from the value or disvalue the voter associates with the election result itself. In the real world, voters with identical valuations of outcomes might well differ in their "sense of responsibility," and the mode of calculating regrets of course makes no allowance for this.)

We have been considering the two-candidate model of Ferejohn and Fiorina: the three-candidate model is simply grotesque. The maximizer of expected utility must estimate *eighteen* subjective probabilities, corresponding to such fantastic outcomes as a two-way tie for first place with the third candidate one vote behind. The authors appear seriously to suggest that a voter may examine the sense of an inequality containing *twenty-four* terms in order to decide whether he should vote for his first or second choice. The poor voter who —perhaps in order to avoid the computational burden—chooses to become a minimax-regret decision maker is faced with an even stranger situation. If he is indifferent between his two favored candidates, his minimax-regret rational behavior is abstention, for any non-zero cost of voting. This occurs because he must recognize the possibility that if he votes for one of his preferred candidates, the other may lose to the disfavored candidate by one vote. The voter thus may incur regret both from his failure to tie the election and from his cost of voting; by abstaining altogether, he may avoid the latter. (I have struggled to see that the result is some sort of technical error, but I do not believe it is.) I will agree to let the decision theorist call such behavior "minimax-regret rational" if he will agree that I may call it silly.

Papers such as this one are frequently no more elegant than they are sensible. For example, the inequalities in the second column of page 534 are very clumsily expressed, about a dozen being used where three or four would suffice (this out of *Caltech*, yet). Conditions (7) and (8) may be summarized: if $k \geq 1/2$, the voter may rationally vote rather than abstain if $c < 1/2 - k/2$; if $k \leq 1/2$, the voter may vote if $c < 1/3 - k/6$; to see this, it helps to make a sketch.

Working through a paper such as "The Paradox of Not Voting" may be a valuable and absorbing exercise, so long as one keeps in mind that one may be dealing with a clay pigeon unable to fly on its own. When such a paper is clear enough to be followed, the models it offers generally show themselves to be deficient in a dozen respects and preposterous in several, and the affirmative claims of such papers are commonly almost wholly spurious. The model presented by Ferejohn and Fiorina neither provides a plausible "theoretical explanation of voting turnout," nor "a means of organizing and explaining the many empirical correlations which exist" (p. 536). We may regret that so few formal papers are so clear, and that so few readers of the *Review* are likely to give such papers the critical examination they deserve.

# Is Minimax Regret Applicable to Voting Decisions?

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In a recent article ("The Paradox of Not Voting: A Decision Theoretic Analysis," *APSR*, 68 [June, 1974], 525–536) John Ferejohn and Morris Fiorina suggest that a (potential) voter may be rational in deciding whether to vote and yet not use the expected-utility calculus introduced for this purpose by Downs and Tullock and extended by Riker and Ordeshook. The authors suggest that a rational voter, in deciding whether to vote, may use the minimax regret criterion due to the late L. J. Savage.[1] Define the regret of an Act A as the difference, if the true state of nature were known (in advance), between the best expected payoff that could be obtained and the expected payoff that would be obtained by act A. Then the minimax regret criterion requires the decision maker to act in such a way as to minimize the maximum regret that he can suffer.

They also discuss the fact that the minimax regret criterion does not require the voter to supply any (objective or subjective) estimates of the probabilities associated with the various outcomes of the election. Thus they feel that it is an attractive criterion for decision making under "uncertainty" as opposed to decision making under "risk."

We first take issue with the notion that a decision must be made either under risk or under uncertainty, for there is an entire continuum of situations (which we shall call quasi-risk situations) interposable between the two. We then argue that the minimax regret criterion is not a reasonable criterion even under complete uncertainty when one does not have an intelligent opponent. Furthermore we shall argue that the problem of deciding whether to vote usually occurs in a quasi-risk situation and that the minimax regret criterion is inappropriate for such situations since it ignores relevant information. We also claim that most voters do not use the minimax regret criterion.

The distinction that the authors make between decision under risk and decision under uncertainty is the usual one that (i) under risk the probabilities associated with the various events that can occur are known or can be estimated by the decision maker in the sense that he can make statements such as "the probability of event A occurring is .35" and (ii) under uncertainty he can say nothing about the probabilities associated with the various events. Since the probabilities of the possible outcomes in an election are not known or easily estimated by the voter, Ferejohn and Fiorina conclude that the decision to vote is not a decision under risk and therefore must be a decision under uncertainty. Consistent with the theory of subjective probability proposed by one of us,[2] we propose that there are many quasi-risk situations in which the decision maker is not able to supply a "sharp" (single value) estimate of the probabilities of various outcomes, but that he is not totally ignorant of these probabilities. In one Bayesian spirit he may be able to put a prior distribution on these probabilities[3] but more often he would be able to suggest an interval in which he feels the probability of a given event lies. This information is not reasonably ignored in the decision process. For example, the problem whether to vote may be one in which the voter can say that "the probability of Nixon's victory is greater than 0.5." If so, then the voter is in a quasi-risk situation.

Suppose, for the moment only, that the decision to vote is made under complete uncertainty. This assumes that the voter does not suspect that the probability of a tie is small when the electorate is large. The minimax regret criterion would not be a reasonable one for the voter to use since it is a conservative strategy in the sense that it protects the voter against an intelligent opponent whose own interests are contrary to the interests of the voter. When playing against an intelligent opponent who has many strategies at his disposal it may be reasonable to protect one's self by minimizing the maximum possible regret. But the outcome of the election is not chosen by an intelligent opponent who is trying to irritate the voter, for the outcome of a tie is not one determined by any one person. Therefore, the minimax regret criterion is apparently not reasonable when deciding whether to vote even if the decision were made under uncertainty.

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[1] Savage (in *The Foundations of Statistics*), preferred the term "loss" to "regret" in this context, but "regret" is now the more standard term because of another technical meaning for "loss."

[2] For example, I. J. Good, *Probability and the Weighing of Evidence* (London: Charles Griffin; New York: Hafners, 1950).

[3] For example, I. J. Good, "Rational decisions," *Journal of the Royal Statistical Society*, series B, *14* (1952), 107–114.

Although we have just supposed temporarily that voting could be considered to be decision making under uncertainty, we actually claim that it is clearly a case of decision making under quasi-risk for the following reasons. Although the decision to vote is not a decision under risk since the voters do not give point estimates of the probabilities of events such as the event of a tie, we claim that the voters do take into consideration their rough estimates of the probabilities associated with certain events such as a "close race" or a "landslide." For the turnout tends to be greater in a close contest than in a landslide. If the average voter has more incentive to vote when the election is "on the fence" then his decision cannot be made under uncertainty.

We agree with Ferejohn and Fiorina that the voter does not make (point) estimates of the probabilities associated with all possible outcomes and then makes a decision under risk. But we feel it at least as unrealistic to assume that the voter ignores his rough ideas concerning the outcome of the election so his decision is not made under uncertainty. A study is needed of the conditions under which it is rational to vote when the decision is under partial risk. Empirical work could be used to determine the type of partial estimates that voters can make of the probabilities associated with various outcomes of the election.

# The Paradox of Minimax Regret

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The introduction of decision making under uncertainty by Ferejohn and Fiorina[1] is an interesting addition to the literature on rational theories of citizen participation. Decision making under uncertainty assumes, however, that the actor has no knowledge of the probabilities of the various outcomes; this is obviously no more true than the assumption of perfect information about these probabilities made in the decision making under risk model. Voters have some, but not perfect, information about the probabilities of at least some of the different possible outcomes.

Specifically, let us look at the two-party case. In Ferejohn's notation, $(p_3+p_4)$ is the probability of an individual's vote making a difference. We might expect a rational citizen to know that this probability is at most minuscule, even if he cannot calculate its exact value. Now, according to the uncertainty analysis (p. 528), a rational citizen might vote because he fears that his vote will make a difference (i.e., the true state of the world is either $S_3$ or $S_4$). But because the potential voter knows that $(p_3+p_4)$ is minuscule, he is in the position of comparing the near certainty of a small loss against the near impossibility of a large gain. But this returns us to the decision-making-under-risk case (except that we are now dealing with a citizen's guess at maximal values of probabilities rather than his best guess of those probabilities). Thus, given the relative magnitudes of the gains and losses and probabilities associated with voting, it is still unlikely that a citizen would choose to vote on purely rational grounds, no matter which form of decision making he uses.

Furthermore, minimax regret has an additional defect. Presumably, Ferejohn and Fiorina's analysis should hold for any decision-making situation that is formally equivalent to the decision of whether or not to vote. Thus, they are in the unhappy position of arguing that one should always forego a near-certain small gain in order to avoid the minuscule possibility of a large loss. In particular, they would have to argue that one should never cross a street to buy a newspaper. A decision-making procedure that produces such consequences clearly has its defects.

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[1] John Ferejohn and Morris Fiorina, "The Paradox of Not Voting: A Decision Theoretic Analysis," *American Political Science Review*, 68 (June, 1974), 525–535.

# The Paradox of Not Voting for Oneself

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In most states it is permitted to write in the names on the ballot, and these write-in candidates can win if they assemble enough votes. Thus, in all elections in which I might conceivably participate, there are at least three candidates: the Republicans, the Democrats, and my own favorite candidate for president—Gordon Tullock. Indeed, using the notation of the Ferejohn and Fiorina article, assuming that we are talking about 1972, I would find $k$ having a value of about .001. Under the circumstances, if I understand the article correctly, I should always vote, and the vote should always take the form of writing in my own name. Further, as far as I can see, this advice can be generalized. Everyone who would really like to be president should vote and write in his own name, because the minimum regret that they produce for three-candidate elections is also correct for the elections in which there are 30 million candidates. The only problem here is that it would, of course, amount to participating in a lottery, and my possible gain from writing in my own name might turn out to be less than the cost of writing it in. Granted that people are willing to buy lottery tickets, even when the various states offering them take very substantial rakeoffs, I do not imagine this would be much of a disadvantage.

This same conclusion could have been deduced from the Casstevens article, which Ferejohn and Fiorina quite properly criticize as being mathematically incorrect.[1] There is, of course, nothing wrong with the Ferejohn and Fiorina mathematics. Nevertheless, the conclusion is such as to raise considerable doubts about whether they have chosen the right assumptions. Like them, I have always felt that the *ad hoc* procedure of putting in a "D" is undesirable.[2] I would like a better solution. It does not seem to me, however, that their solution is really better.

It is possible to sketch out at least a partial solution to this problem. Clearly the cost of voting is very slight. Under the circumstances, individuals would not be well-advised to invest large resources in determining whether the conventional wisdom (which they picked up in school) about the duty and value of voting is correct. Further, in the present state of the literature, it must be admitted that finding out that voting is not a paying proposition would be quite expensive. Under the circumstances, people making decisions about whether or not to expend resources on *investigating* whether or not they should stop voting might, quite rationally, decide not to make the *investigation.* This explanation for widespread voting is, of course, in addition to the usual social pressure arguments for voting, and the other conventional argument that many people have been indoctrinated in such a way that they get a positive pleasure from voting.

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[1] Thomas Casstevens, "A Theorem About Voting," *APSR,* 62 (March, 1968), 205–207.

[2] The assumption that $C_v$ takes a minus value is made in Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: University of Michigan Press, 1967), p. 110.

# Closeness Counts Only in Horseshoes and Dancing

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## Introduction

During the period in which our article (*APSR* vol. 68 [June 1974]) circulated in manuscript form it provoked an unusual amount of collegial reaction. Of course, we were quite prepared for a reaction from those who use decision-theoretic models in their research—they were our intended audience. More surprisingly, we also received comments from less directly involved bystanders—a medieval historian for example. All this correspondence indicates to us that nearly everyone has his own theory of how voters behave, and that most such theories do not agree with the one presented in our article. The comments of Professors Tullock, Beck, Mayer and Good, and Stephens further support this conclusion.

In an appendix to this note we have responded to the imaginative point raised by Tullock. As for the traditional questions raised by our other critics, however, we adopt a different line of rebuttal. Rather than conduct an unfruitful debate over the *a priori* plausibility of the minimax regret model we will do something that theorists too seldom do: examine some data. Before doing so we will make an important distinction between using a model prescriptively and using it descriptively. (Decision-theoretic types tend to move a bit too easily from one usage to the other.) Then, after reviewing the major point of our article we will turn to the data.

## Prescription and Description

Much of the literature in decision theory reflects a search for optimal decision procedures—those which an intelligent, purposeful being *should* follow to make the "best" decision relative to his values and beliefs. Many theories of decision making found in the literature are avowedly *not* descriptive theories. Savage, for example, always viewed his theory of subjective expected utility maximization as having primarily prescriptive value.[1] Raiffa expresses an identical view.[2] The theory can be used to *guide* a decision maker, to enable him to make better decisions than he might were he to use his intuitive judgments. Thus, to take some particular theory of decision making and assert that it describes the behavior of ordinary people obviously entails a large leap into faith, or to put it more professionally, a very bold hypothesis.[3]

Those who apply decision-theoretic models to the study of politics sometimes blend the different usages of these models, now rejecting a model as descriptive because it seems unreasonable as a prescriptive model (e.g. minimax regret), then accepting a model as descriptive because it seems reasonable as a prescriptive model (e.g. expected utility). Consider some excerpts from the Mayer-Good comment:

> . . . we shall argue that the problem of deciding whether to vote usually occurs in a quasi-risk situation and that the minimax regret criterion *is inappropriate for such situations since it ignores relevant information.* We also claim that most voters *do not use* the minimax regret criterion [our emphasis].

The first sentence asserts that the minimax regret criterion is unacceptable as a prescriptive model, i.e., rational voters *should not* behave that way. The second sentence advances an empirical claim—that voters *do not* behave as minimax regretters. Again,

> The minimax regret criterion *would not be a reasonable* one for the voter to use since it is a conservative strategy in the sense that it protects the voter against an intelligent opponent whose own interests are contrary to the interests of the voter. . . . Therefore the minimax regret criterion is apparently *not reasonable* when deciding whether to vote, even if the decision were made under uncertainty.

Here, Mayer and Good are rejecting the model because they believe it to be "unreasonable," not descriptively wrong. But later they shift back:

> . . . the turnout tends to be greater in a close contest than in a landslide. If the average voter has more incentive to vote when the election is "on the fence," then his decision cannot be made under uncertainty.

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[1] Leonard J. Savage, *The Foundations of Statistics,* 2nd rev. ed. (New York: Dover, 1972), pp. 19–21, 59, *passim.*

[2] Howard Raiffa, *Decision Analysis* (Reading, Mass.: Addison-Wesley, 1968), p. x.

[3] A number of recent experiments have cast serious doubt on the descriptive value of prevailing formulations of expected utility models. See, for example, two articles by Sarah Lichtenstein and Paul Slovic. "Reversals of Preference Between Bids and Choices in Gambling Decisions," *Journal of Experimental Psychology,* 89 (July, 1971), 46–55. And "Relative Importance of Probabilities and Payoffs in Risk Taking," *Journal of Experimental Psychology Monograph,* 78 (December, 1968), 1–18. Also, Kenneth R. MacCrimmon, "Descriptive and Normative Implications of the Decision Theory Postulates," in *Risk and Uncertainty,* ed. Karl Borch and Jan Mossin (London: Macmillan, 1968), pp. 3–32.

Here we have an empirical claim clearly subject to falsification.

Similarly, consider Beck's and Stephens's comments. Upon first reading they appear to be rejecting the minimax regret model on empirical grounds. People do cross streets and drive on freeways when there is some nonzero probability of being wiped out by doing so. Such behavior is contrary to a minimax regret analysis of these situations. But again, such arguments reflect an implicit assumption arising from the prescriptive decision-theoretic literature; namely, that the domain of a decision theory is universal, that people should decide according to a given model in any and all contests. Such generality would be nice. Unfortunately, recent experimental work by Charles Plott and Michael Levine raises the messy possibility that individuals act as if they vary their decision rules in response to the decision context (i.e., the available alternatives and the consequences).[4] We agree with Beck and Stephens that minimax regret fails as a descriptive model of freeway driving behavior, but this observation in principle bears no relation to the accuracy of minimax regret as a descriptive model of voting behavior.[5]

To summarize our position, then, we are prepared to joust with our critics about the descriptive accuracy of various decision-theoretic models of voting behavior. But we reject the notion that a descriptive theory can be rejected on the grounds that the model underlying it appears implausible when advanced as a *prescriptive* theory. After all, the American electorate does not have the benefit of televised election eve lectures by Professors Mayer, Good, Beck and Stephens on the subject of reasonable voting behavior. How the citizenry behaves and how our fellow professors believe it should behave are two different questions. Our concern is only with the former.

### What Did We Say?

Although Professor Stephens compliments our "extraordinarily clear contribution," we apparently were not quite clear enough. In the first paragraph of our article we wrote

> . . . whatever their ability to account for such general phenomena as candidate strategy and electoral outcomes, many believe that rational choice models do not provide a satisfactory explanation for the most basic political decision in a democratic system: to vote or not to vote. The purpose of this paper is to show *one means* of rescuing rational choice theorists from this embarrassing predicament [our emphasis].

Several pages later we wrote

> In this section, we will demonstrate that one well-known classical procedure yields a quite weak condition for voting vis-à-vis abstaining.

Finally, in our conclusion we wrote

> Rational-choice theorists are guilty of equating the notion of rational behavior with the rule of maximizing expected utility. Alternatively, we usually define the first as the second. Yet although the rule of maximizing expected utility is the most widely known, widely used and widely accepted rationality criterion, it is not the only one.

As the preceding comments indicate, the major point of our article is simply to remind some of our colleagues that rational behavior is a much broader notion than expected utility maximizing. To pose the alternatives as either expected utility maximizing or ". . . the mysterious and inexplicable world of the irrational" is far too stark a contrast.[6] In our article we simply offered another alternative without necessarily implying that it was the only additional possibility. This is still our position. We are not so bold as to argue that all voters literally *are* minimax regret decision makers. We argue only that the minimax regret model of voting behavior is a model which has at least one redeeming feature: it does not make the patently wrong prediction (universal abstention) that the Downsian expected utility model does.

Moreover, the minimax regret model can be differentiated empirically from the Downsian expected utility model. The minimax regret model is one member of a class of models in which $B$ matters but not $p$. The Downsian expected utility model is a member of a class of models in which the product $(pB)$ matters. In the remainder of this note we will evaluate these two classes of models using survey data on the American voter. The critics say that it is unreasonable for people to ignore the probability that they affect the election outcome when they vote. But what does the evidence say?

### Analysis

The Downsian expected utility model specifies that a citizen should vote if

$$(p_3 + p_4)(B/2) > c,$$

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[4] Charles Plott and Michael Levine, "On Using the Agenda to Influence Group Decisions: Theory, Experiments and an Application," (mimeograph: California Institute of Technology, 1974).

[5] As we indicated earlier, examples such as crossing streets and driving on freeways are traditionally held up against minimax decision rules. For a discussion see R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: Wiley, 1957), pp. 278–282. Professors Beck and Stephens advance such criticisms as if we were unaware of them. Not so. We simply regard these criticisms as not telling when one works in the descriptive mode.

[6] William Riker and Peter Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25.

where $p_3$ is the probability a citizen breaks a tie, $p_4$ is the probability he creates a tie, $B$ is his party differential, and $c$ his cost of voting. As we noted in our article, virtually no one votes if this model holds. Thus, additions such as the Riker-Ordeshook $D$ term must be postulated if one is to take the model seriously for predictive purposes.

In contrast, the basic minimax regret model specifies that a citizen should vote if

$$B/4 > c$$

Clearly, this condition will hold for any citizen with a nontrivial party differential, unless his costs of voting are unusually high (e.g., blacks in the South until recently).

For purposes of differentiating between the two models empirically, a simple test may be performed. Under the expected utility model, perceived closeness of the election affects the outcome; under the minimax regret model, it does not.

To elaborate, in the Downsian expected utility model closeness of the election interacts with the party differential. If the citizen's party differential is trivial, closeness should have little or no effect on his decision. Similarly, if he does not see the election as at all close, the size of the party differential should have little effect. The main effect should occur when both a nontrivial party differential *and* a perception of a close election occur simultaneously.[7] For the minimax regret model, however, closeness of the election is irrelevant. As a citizen's party differential increases, the likelihood of his voting increases, irrespective of $p$.

Thus we have the following empirical hypotheses to test:

H—EU: (Expected utility) turnout increases as the product ($pB$) increases.

H—MR: (Minimax Regret) turnout increases as $B$ increases.

Naturally, between theoretical variables and empirical indicators a large gap intervenes. We cannot hope to measure $p$ and $B$ very precisely. But data exist which allow a rough test of these hypotheses, and given that proponents of expected utility models previously have employed these data, we feel justified in following suit.

The data are drawn from the 1952 to 1964 presidential election surveys conducted by the University of Michigan SRC.[8] In each survey there is a question which asks a respondent how close he believes the election to be. As a proxy for $p$ we took the answers to this question and dichotomized them into close and not close.[9]

Our measures of the party differential varied across elections. For the 1960 and 1964 elections we took the answers to the most-important-problems questions, weighted them by the respondent's professed degree of concern, and assigned a polarity according to which party could best handle the problem. Thus, the party differential for these years reflects issue concerns almost exclusively.[10] The most-important-problems questions were not asked prior to 1960. Thus we followed Riker and Ordeshook in considering the respondent's degree of concern over the electoral outcome as an estimate of $B$ for 1952 and 1956.[11]

As a measure of turnout should one use pre-election intentions, or post-election reports? The first are noticeably higher than the second, yet the second are elicited at a point in time considerably removed from a respondent's estimates of $p$ and $B$. Seeing no conclusive argument for either, we have examined both.

Finally, we have examined turnout among *registered* voters only (i.e., those self-reported as registered). The models under consideration predict variations in abstention rates. But if a citizen is not registered, we have no way of knowing whether or not he would have voted had he been eligible. Some might contend that a decision not to register should be considered equivalent to a decision not to vote. But we do not view the matter so simply, particularly in reference to a time when registration requirements were more stringent than today. The models under consideration predict variations in abstentions when voting is an available alternative. Tests of such models should not depend on citizens with low party differentials or low $p$-terms concentrating among the legally ineligible.

Having outlined these numerous practical decisions, we now proceed to the data. We will evaluate the data by formulating two statistical models. The first specifies the turnout percentages one should observe under the Downsian expected utility model, namely a pure interaction effect of the following nature:

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[7] Indeed, Angus Campbell et al. claim that such an interaction occurs in 1956. See *The American Voter* (New York: Wiley, 1960), Table 5-3, p. 99.

[8] The data analyzed in the table were made available by the interuniversity consortium for political research. The data were originally collected by the SRC political behavior program. Neither the original collectors of the data nor the consortium bear any responsibility for our interpretation of the data. The 1968 survey is not used in the following analysis because of the three-candidate race. We do not possess the 1972 data.

[9] Respondents' perceptions of closeness of the election are coded into four main categories ranging from landslide to tie. We have collapsed these categories into not close (win by landslide, or win by quite a lot) and close (win by a little, even). We assume that $p$ tends to be lower for the first category of respondents.

[10] In the analysis which follows the full range of the scale is not used. Respondents simply are dichotomized into zero scorers and everyone else.

[11] These responses, too, were dichotomized. Those who responded "don't care," or "care a little" were considered low party differential, "care," and "care a lot" as high party differential.

| | | Party Difference | |
|---|---|---|---|
| | | No | Yes |
| Closeness | Not Close | $\gamma$ | $\gamma$ |
| | Close | $\gamma$ | $\gamma+\beta$ |

That is, if there is no party difference, variations in closeness should produce no increase in the likelihood of voting. Similarly, for those who think that the election is not close, increasing party differences should have no impact.

The second model specifies the turnout percentages one should observe under the minimax regret model, namely, a pure column effect:

| | | Party Difference | |
|---|---|---|---|
| | | No | Yes |
| Closeness | Not Close | $\gamma$ | $\gamma+\beta$ |
| | Close | $\gamma$ | $\gamma+\beta$ |

Increasing party difference should increase the likelihood of voting independently of the perceived closeness of the election.

Unfortunately, our task is complicated by the fact that neither of the preceding models is a special case of the other: they are not "nested." In such cases there is no generally accepted method of constructing a test statistic such that if it exceeds a prespecified bound one model is accepted, and if not, the other is accepted. Consequently, we employ a methodology which we believe to be reasonable, although *ad hoc*. The procedure is to nest both models in a more general model, fit the general model to the data, then test hypotheses to see if we can reject one of the two specific models of interest.

The general model is

| | | Party Difference | |
|---|---|---|---|
| | | No | Yes |
| Closeness | Not Close | $\gamma$ | $\gamma+\beta_2$ |
| | Close | $\gamma$ | $\gamma+\beta_1$ |

In terms of this table the two hypotheses of interest are

$H_0$: $\beta_2=0$, acceptance⇒rejection of minimax regret.

$H_1$: $\beta_1=\beta_2$, acceptance⇒rejection of Downsian expected utility maximization.

If we can reject exactly one of the two hypotheses, we have a reasonable case for deciding in favor of one model rather than the other. Evidently, the problem with this procedure is that we might be unable to reject either model, or we might find grounds for rejecting both. Should either of these possibilities come to pass we can draw no firm conclusion.

Parameter estimation proceeds as follows. The dependent variable is

$$Y=\begin{cases}1 & \text{if respondent is a voter.}\\ 0 & \text{if respondent is a nonvoter.}\end{cases}$$

And the independent variables are

$$X_1=\begin{cases}1 & \text{if respondent has a party differential.}\\ 0 & \text{otherwise.}\end{cases}$$

$$X_2=\begin{cases}1 & \text{if respondent perceives the election}\\ & \quad\text{as close}\\ 0 & \text{otherwise.}\end{cases}$$

The general model is

$$\begin{aligned} Y &= \gamma+\beta_1X_1X_2+\beta_2X_1(1-X_2)+\epsilon\\ &= \gamma+(\beta_1-\beta_2)X_1X_2+\beta_2X_1+\epsilon\\ &= \gamma+\alpha_1X_1X_2+\alpha_2X_1+\epsilon, \qquad (1)\\ &\qquad \text{where } \alpha_1=(\beta_1-\beta_2)\\ &\qquad\qquad \alpha_2=\beta_2. \end{aligned}$$

Using ordinary least squares, we estimated the parameters of (1) and utilized these estimates to form a consistent estimate of

$$\hat{Y}=\hat{\gamma}+\hat{\alpha}_1X_1X_2+\hat{\alpha}_2X_1.$$

The latter can be interpreted as the probability that $Y=1$ conditional upon the observed value of $X_1$ and $X_2$. Because $Y$ takes on only two values, $\epsilon$ is distributed as a Bernoulli variable with variance estimated consistently by $\hat{Y}(1-\hat{Y})$. These variance estimates were utilized to approximate the variance–covariance matrix of $\epsilon$. Then equation (1) was re-estimated. This generalized least-squares procedure provides consistent and unbiased estimates of the coefficients $\alpha_1$, $\alpha_2$, and their standard errors.

In Table 1 we present the basic turnout data for the four presidential elections examined. A cursory examination of these tables produces at least two observations. First, one notices immediately that turnout rates among registered voters are very high for all values of the independent variables. Nearly everyone *intends* to vote, and ap-

**Table 1. Percentage Voting as a Function of Party Differential (PD) and Closeness of Election**

| | | *Pre-Election Intention* | | *Post-Election Report* | |
|---|---|---|---|---|---|
| | | Low PD | High PD | Low PD | High PD |
| *1952* | NC | 94%<br>(81) | 99+<br>(291) | 85<br>(72) | 93<br>(268) |
| | C | 95<br>(179) | 99<br>(446) | 87<br>(171) | 92<br>(416) |
| *1956* | NC | 95<br>(81) | 98<br>(197) | 82<br>(81) | 87<br>(200) |
| | C | 95<br>(198) | 98<br>(515) | 86<br>(203) | 92<br>(517) |
| *1960* | NC | 94<br>(36) | 100<br>(124) | 88<br>(34) | 92<br>(123) |
| | C | 99+<br>(216) | 99<br>(749) | 94<br>(207) | 95<br>(711) |
| *1964* | NC | 95<br>(146) | 99<br>(369) | 86<br>(140) | 92<br>(353) |
| | C | 98<br>(146) | 98<br>(326) | 87<br>(138) | 92<br>(326) |

proximately 90 per cent of the respondents report *having voted*, although this figure presumably overestimates actual turnout.[12] Second, although the independent variables do not seem to produce much variance in the turnout rate (given the mean, of course, they cannot), there does seem to be some systematic variation in turnout which is related to the party differential but not to closeness of the election.

More exactly, in Table 2 we show the coefficient estimates for equation (1), with standard errors in parentheses. Our test procedure is as follows: for each data set we reject both models if the general model is rejected at the .05 level using a $\chi^2$ test. If the general model is not rejected, we reject $H_0$ (i.e., do *not* reject minimax regret) if $\hat{\alpha}_2>0$ at the .05 level; and we reject $H_1$ (i.e., do *not* reject Downsian expected utility) if $\hat{\alpha}_1\neq 0$ at the .05 level.

Of the eight data sets, we fail to reject the general model in seven cases. The general model fails for the 1960 pre-election data. Interestingly, the general model fails in this case because of the presence of a closeness of the election effect in the zero party differential column. The minimax regret model cannot explain this, nor can the Downsian expected utility model in which the expected party differential equals $p\cdot 0=0$.

Of the seven cases in which the general model could not be rejected, we found that $H_0(\hat{\alpha}_2=0)$ could be rejected in five cases but $H_1(\hat{\alpha}_1=\hat{\alpha}_2)$ could be rejected in only one case.[13] Thus, of seven cases in which the general model is not rejected, the Downsian model of expected party differential survives only once (1956 post reports) while the minimax regret model survives five times (1952 pre- and post-, 1956 pre-, 1964 pre- and post-). Note the perfect minimax regret pattern for the 1956 pre-election data, and the near-perfect patterns for the 1952 pre- and 1964 post-election data. In sum, for these data the minimax regret model has a 5 to 1 advantage over the Downsian expected utility model. The obvious conclusion is that the Downsian model of *expected* party differential turns in a dismal performance as a descriptive turnout model, while the minimax regret model fares considerably better. According to the data, the party differential affects turnout *independently* of expected closeness.[14] While this relationship is inconsistent with the Downsian suggestion, it is consistent with the minimax regret model and with *numerous other models* one might construct in which the party differential affects turnout independently of the probability of affecting the outcome.

A final caveat: in principle one might accept the notion that voters maximize expected utility while one simultaneously rejects the Downsian formulation of an expected utility model. If one is disposed to this view, our analysis could be interpreted as showing that while the Riker-Ordeshook modification of Downs successfully rescues the Downsian model from the prediction that no one votes, still further modifications are required. A natural candidate is a model in which the private benefits of voting (i.e., those like the $D$ term which are not discounted by $p$) vary with the party differential. (We might note here that Strom's expected utility model is not such a model—in it, party differential terms still are multiplied by $p$ terms—see Appendix 2.) One

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[12] Aage Clausen, "Response Validity: Vote Report," *Public Opinion Quarterly,* 32 (Winter, 1968–69), 588–606.

[13] In the remaining case (1960 postelection reports) neither hypothesis can be rejected. Thus although the general model is not rejected for this case, both particular models are.

[14] At least if one considers the whole sample of registered voters. We might note that Fiorina has developed a "hybrid" expected utility model of voting behavior which produces the curious result that a subclass of the citizenry is *less likely* to vote the closer they perceive the election to be. Now, if the world were evenly divided between those whose turnout was encouraged and those whose turnout was discouraged by a high $p$, one would expect no aggregate effect. Empirically, however, the unusual subclass to which Fiorina's prediction applies typically appears to be small. See Morris Fiorina, "The Voting Decision: Investment and Consumption Aspects," (California Institute of Technology: Social Science Working Paper No. 46).

Table 2. Estimates of Equation 1

| Data Set | $\hat{\alpha}_1$ | $\hat{\alpha}_2$ | $\gamma$ |
|---|---|---|---|
| 1952 pre | −.006 (.006) | .050 (.014)* | .946 |
| 1952 post | −.001 (.020) | .069 (.027)* | .864 |
| 1956 pre | −.005 (.012) | .030 (.016)* | .950 |
| 1956 post | .047 (.027)* | .021 (.032) | .849 |
| 1960 pre$^{\text{n.s.}}$ | −.015 (.009) | .012 (.010) | .988 |
| 1960 post | .032 (.026) | −.015 (.029) | .934 |
| 1964 pre | −.013 (.010) | .023 (.012)* | .966 |
| 1964 post | −.001 (.021) | .055 (.025)* | .863 |

* $p < .05$.
n.s. general model rejected, $p > .05$.

or more theories constructed along these lines would survive our statistical test, for given the data at hand they are indistinguishable from the minimax regret model. In such situations there is only one way to proceed: build the alternative models, then collect the data which will allow discrimination between them and the minimax regret model of the voting decision.

## Conclusions

Our critics have attacked the minimax regret model primarily on the grounds that they do not *feel good* about it, that it is *unreasonable*, that voters just *shouldn't* behave that way. Such attitudes are understandable among statisticians and operations researchers. In political science, however, good feelings in one's tummy are not considered persuasive empirical evidence. As we have tried to demonstrate, the available data provide no empirical basis for rejecting the minimax regret model *as a descriptive model of the turnout decision*, regardless of its weaknesses as a prescriptive theory of decision making. One cannot be so generous in one's evaluation of the central component of the traditional Downsian model. We would be the first to recognize the extreme crudeness of our data and the necessarily tentative nature of the empirical test. But whatever its flaws, our data analysis is far more substantial than that provided by the critics, whose closest brush with the real world consists of an analysis of freeway driving decisions.

To re-emphasize the main point of our original article, what we tend to accept as rational may blind us both to alternative standards of reasonable behavior and to what is actually happening in the political world. The Downsian turnout model has lived on not because it has any descriptive value, but rather because theorists who accept the prescriptive model underlying it refuse to let it die. In proposing a minimax regret model we are not attempting to erect a new graven image. We are only attempting to fan the fires of agnosticism.

## Appendix 1: On Tullock

As one would expect, Professor Tullock offers an ingenious criticism. We offer a two-part reply. First, there is the purely technical point that in our article we assumed that the cost of voting for candidates 1, 2, or 3 is a constant. But to cast a write-in vote in an otherwise two-candidate election presumably entails a higher cost than pulling a lever. Hence, the result to which Tullock objects does not generally hold in the write-in context. Second, we would dispute Tullock's basic premise that every citizen is his own first choice for president. Rather, we wager that the citizen who ranks himself as his most preferred candidate is rare. Our mini-poll of Caltech faculty (not noted for poor self-images) produced no cases. Thus, we suggest that Professor Tullock is a deviant case and can safely be ignored for purposes of analysis.

## Appendix 2: On Strom

Strom's piece constitutes a rather desperate attempt to save the Downsian expected utility model. The dénouement of Strom's argument is condition 9:

$$\text{Is } P_{2j}(U_{2j}^{v})+P_{3j}(U_{3j}^{v})+Q_{3j}(U_{3j}^{a})+Q_{4j}(U_{4j}^{a}) > C+P_{4j}(U_{4j}^{v})+Q_{2j}(U_{2j}^{a})?$$

Now, Strom would have us believe that the right-hand side tends to $C$ while the left-hand side adds to some significantly positive quantity. Nonsense. We cheerfully admit that the right-hand side tends to $C$, but just as cheerfully contend that the left-hand side tends to 0. Strom can't have his cake and eat it too. He must demonstrate empirically that the utility terms on the left-hand side are so large that even when discounted by $p$-terms, the resulting sum of products is nontrivial. We await such demonstration.

# The Ethical Voter

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No rational egoist should bother voting because, as Skinner's Dr. Frazier notices, the probability of any one man casting the decisive ballot "is less than the chance he will be killed on the way to the polls."[1] No matter how deeply he cares about the electoral outcome, a man must realize that his vote is only one among very many. The larger the electorate, the smaller the probability of any one vote's changing the outcome, so in most modern polities the politically rational thing to do is to conserve on shoe leather.

Real-world voters, of course, do flock to the polls, which is usually explained in terms of a feeling of "civic duty." The fact that men get some satisfaction from discharging their civic duty by voting might answer the question of why it is rational for them to go to the polls. Unfortunately, it leaves another—how do they vote once they get there? Presumably, they go ahead and vote according to the dictates of their egoistic interests.[2]

Ferejohn and Fiorina make a valuable contribution by initiating the search for an alternative motivational basis for the phenomenon of voting.[3] However, their own solution, "minimax regret" (choose the alternative having the least unbearable worst possible outcome), is seriously flawed. As Ferejohn and Fiorina argue, voting must be either an investment or a consumption decision if it is to be rational for Economic Man. But if it is an investment decision one would expect a rational actor to use the same decision rule when voting as when taking other investment decisions such as portfolio choice; and from the study of these other decisions it is clear that investors do not aim at minimax regret. Indeed, there is no rational reason they should. Ferejohn and Fiorina rely on the argument, "My God, what if I didn't vote and my preferred candidate lost by one vote? I'd feel like killing myself." But before putting in the knife, the non-voter should recall that "what if" arguments are decisively refuted by "seemed like a good idea at the time" rejoinders. A multitude of decisions which one comes to regret *ex post facto* were *ex ante* the only reasonable courses of action. Most of us wish we had bought Xerox stock when it was first issued; J. P. Morgan always regretted rejecting young Henry Ford's request for him to finance the original Model T venture; etc. What are clearly mistakes with the benefit of hindsight were nevertheless good moves at the time the decision had to be taken. Everyone regrets that he is not prescient, that is all. These regrets are unaltered by minimaximizing.

The remaining possibility is that voting is a consumption decision. Of course, consumer choice *is* an important topic in economic theory. While emphasizing the fact, however, Ferejohn and Fiorina overlook important differences in the focus of consumer and investment theory. The economist can say why an investor likes his portfolio (it is instrumental; it will pay off in terms of other goods) but he cannot say why a consumer fancies his purchases. The theory of consumer choice predicts how Economic Man will trade off one good for another under various market and budgetary conditions, and it does so by taking as given the preferences of consumers. In the voting application, consumer theory might help to explain why people fail to vote when the price is high (e.g., on a rainy day), but there must be a prior assumption that people think voting is a good, i.e., that they prefer voting to nonvoting *ceteris paribus*. This leaves the question of why they like to vote, which presumably takes us back to civic duty or irrationality.

The main thrust of this comment is meant to be constructive rather than destructive. It follows in the spirit if not in the footsteps of Ferejohn and Fiorina. Essentially, the purpose is to suggest another reason that voters should go to the polls and, more significantly, another analysis of what they do once they get there. The argument has implications both for party strategies and for the interpretation of electoral results.

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[1] B. F. Skinner, *Walden II* (New York: Macmillan, 1948), p. 265. See also G. F. W. Hegel, *The Philosophy of Right*, trans. T. M. Knox (Chicago: Encyclopedia Brittanica, 1952), p. 104; Brian Barry, *Political Argument* (London: Routledge & Kegan Paul, 1965), pp. 328–30 and *Sociologists, Economists and Democracy* (London: Collier-Macmillan, 1970), pp. 19–23; and Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: University of Michigan Press, 1967), chap. VII.

[2] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), chap. 14. Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963).

[3] John A. Ferejohn and Morris P. Fiorina, "The Paradox of Not Voting: A Decision Theoretic Analysis," *American Political Science Review*, 68 (June, 1974), 525–36.

The keystone of the analysis is the assumption that individuals have both egoistic and ethical preferences, that their welfare functions have both egoistic and ethical components. Nothing very precise need be said about the relative strengths of these motives. Ordinarily egoistic preferences ride roughshod over ethical ones, but no matter. All that is required for present purposes is that ethical preferences carry *some* weight, that they will be decisive at least where everything else is equal. Clearly, such a lexicographical relationship seriously underestimates the strength of ethical preferences. They are not mere tie-breakers. Rather, strong ethical preferences can often override weak egoistic ones. The assumption which this argument requires, then, is a very weak one.

Such a description of human nature is reminiscent of the theories of David Hume, who holds that an individual is capable of displaying benevolence, at least when his own interests are not at stake. The ethical theories of Hume's friend Adam Smith are identical on this point as evidenced by his discussion of the "impartial spectator." More recently John Harsanyi has worked with similar assumptions.[4]

An important distinction between egoistic and ethical preferences is that the former are strictly effect-oriented, whereas the latter applaud trying as well as succeeding. The vote of a single man does no more to enact his ethical preferences than it would to enact his egoistic preferences, but the demonstration that his motives are pure and his heart is in the right place counts for something ethically speaking. Thus, one reason men go to the polls might be to take a moral stand. Not very much depends on this alternative explanation of why men vote, however. The balance of the argument is equally valid if men go to the polls purely out of a sense of civic duty. There is certainly nothing inconsistent in arguing that a deontological tug gets citizens into the poll booth and that ethical preferences then take over.

Precisely the same factor that makes it irrational for an egoist to go to the polls makes it rational for a less than thoroughly self-centered man to vote his ethical preferences once there. As one in a multitude, his vote cannot much affect the outcome. Since there is very little he can do inside the poll booth to further his egoistic interests, a man is freed to vote his ethical preferences. The larger the electorate, the more pronounced this tendency. When the curtains of the poll booth are drawn, then, a voter assumes the air of Adam Smith's impartial spectator. It should also be noted that this is an extraordinarily cheap way for a man to assuage his conscience. Casting a ballot for the forces of The Good is far safer than casting a stone.

Put into a more general form, this analysis suggests that political man behaves ethically whenever the losses he expects from so doing are low enough to be outweighed by the rewards. This implies at least two dimensions—one, the stakes in the decision (the cost if it goes against him or the reward if he wins); and the other, his chances of altering the outcome. A man is safe in behaving ethically when there is not much in it for him (cells II and IV in Figure 1). When he has much at stake he certainly longs to tip the outcome in his favor, and he will try to do so if he has the power (cell I) but lacking the power he simply sighs while choosing the ethical course of action (cell III). In this form the theory is amenable to empirical examination. Provisional hypotheses would be that, controlling for his stake in the project, the stronger the respondent's sense of political efficacy, the less likely he is to opt for the policy he perceives to be the fairest; and, controlling for efficacy, the higher the respondent's stake in the program, the less likely he is to favor the fairest policy. A further refinement might involve controlling for the ethical significance of the issue; some are central to a man's sense of fairness, whereas other issues might be very peripheral. Even in testing the hypothesis as stated, two additional considerations must be kept in mind. The logic of the first hypothesis obviously implies a curvilinear correlation. The difference between a one-in-a-million and a one-in-ten-million chance of altering the outcome is clearly much less significant for the rational voter than the difference between a one-in-a-hundred and a one-in-a-thousand chance, although the difference in each case amounts to a factor of ten. And, when costs are allowed to vary, as in the second hypothesis, an individual's attitude toward risk-taking be-

| | | Stake | |
|---|---|---|---|
| | | high | low |
| Efficacy | high | I egoistic | II ethical |
| | low | III ethical | IV ethical |

**Figure 1. Sources of Ethical Voting Behavior**

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[4] David Hume, *A Treatise of Human Nature* (London: John Noon, 1739), especially Book III, and *An Enquiry Concerning the Principles of Morals* (London: T. Cadell, 1777). Adam Smith, *The Theory of Moral Sentiments*, 6th ed. (London: A. Strahan and T. Cadell, 1790). John C. Harsanyi, "Cardinal Welfare, Individualistic Ethics and Interpersonal Comparisons of Utility," *Journal of Political Economy*, 63 (August, 1955), 309–21.

comes important. The hypothesis as stated assumes that his taste for risks is a constant, but it is easy enough to build into the hypothesis any other theory about how it might vary.

Two politically significant conclusions follow from this recognition of the importance of the ethical component in electoral choice. First, political parties stand to gain more popular support through a moralizing crusade (provided, of course, that they choose popular causes) than by focusing debates on questions of who gets what when, and how. Second, electoral results should be interpreted as reflecting essentially ethical preferences insofar as partisans have structured the debate in such a way as to allow these to emerge. Both these conclusions, it should be emphasized, are predicated on the explicit assumption about human nature and on the implicit assumption that voters pursue their goals as rational means-ends calculators.

These principles are rather more familiar to politicians than to political scientists. Lyndon Johnson's Great Society, George McGovern's schemes for income redistribution and Harold Wilson's Social Contract are all appeals for mandates from those who are comfortably placed in society for the commitment of social resources to help those in desperate straits; and two of the three succeeded in persuading enough voters to forsake self-interest to form a government. Something approaching recognition of these principles is found in the Banfield and Wilson "ethos theory," although one hastens to add that both public- and private-regardingness can qualify as ethical behavior. What matters is not the substance of the rule of conduct but rather how it is justified, and so long as private-regarding citizens argue in terms of universalizable prescriptions (e.g., the Puritan Ethic), their selfishness must be considered an ethical principle.[5]

The argument of this comment is best summarized as Niebuhr's slogan set on its head: immoral men, moral society.[6] This, however, only refers to the inclinations of actors. While individuals are more tempted to behave selfishly in their personal dealings, formal legal restraints or informal social ones may deny them the opportunity. In their public roles, men can vote their ethical preferences only if these alternatives are listed on the ballot.

This argument is not to deny Niebuhr's observation that modern mass society has some unsavory characteristics, among the foremost of which is intolerance. The present analysis suggests that intolerance does not proceed from a lack of idealism but rather from an excess of it. In *Freedom and Reason* Hare shows that the moral fanatic is just one special variety of moralist—one which is repugnant from most moral perspectives, admittedly, but a moralist nonetheless.[7] Most men, while far from being fanatics, are less flexible on ethical issues than they would be on material demands. This is surely because their ethical preferences are more intensely held. Hence, even in a well-integrated society in which men are very close together on ethical matters, such differences as remain might matter far more to them than those arising out of egoistic desires. The judgment must be a discouraging one: the fact that ethical preferences are reflected in electoral results is not necessarily a cause for celebration. One cannot help wishing that Hitler had forsaken ethical ideals and pursued narrow material gain instead.

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[5] Edward C. Banfield and James Q. Wilson, "Public Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, 63 (December, 1964), 876–87. Another example might be John H. Fenton, *The Catholic Vote* (New Orleans: Hauser Press, 1960); religious-group voting surely has both ethical and reference group aspects. R. M. Hare, *The Language of Morals* (Oxford: Clarendon Press, 1952) argues that the essential characteristic of moral statements is that they are universalizable prescriptions.

[6] Reinhold Niebuhr, *Moral Man and Immoral Society, A Study in Ethics and Politics* (New York: Scribner's, 1932), pp. xi-xii, writes, "In every human group there is less reason to guide and check impulse, less capacity for self-transcendence, less ability to comprehend the needs of others and therefore more unrestrained egoism than the individuals, who compose the group, reveal in their personal relationships."

[7] R. M. Hare, *Freedom and Reason* (Oxford: Clarendon Press, 1963).

# The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities*

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## Introduction

Vote trading, or logrolling, is a concept familiar to students of legislative bodies. Such students of the formal properties of voting bodies—James M. Buchanan, Gordon Tullock and James S. Coleman, in particular—have extolled the strategy of exchanging votes in a legislative arena as a means by which individual members are able to maximize their total utility.[1] Their scenario is straightforward: a legislator who does not feel very strongly about an issue he would oppose should be willing to trade votes with another member in order to secure the passage of a motion he strongly favors, but which would otherwise be defeated. Exchanging of votes thus seems analogous to exchanging of commodities for money in a market economy. William H. Riker and Steven J. Brams, however, have demonstrated that, for a legislature of three or more members and at least six motions, some trades which appear to be individually rational do not maximize the total utility of the traders.[2] Instead, there is a "paradox of vote trading" in which the incentive for every pair of legislators to trade is present, but the effects of externalities (costs imposed on the nontrader for each pair of motions) reduces the total utility to each player from what he could expect if no trades were made.

Riker and Brams argue that the advocates of vote exchanges have failed to consider the effects of externalities.[3] An externality is simply any cost which is imposed on an individual as a result of actions not under his control. In the case of the paradox of vote trading, such externalities considered by Riker and Brams are always "external costs"—i.e., the trading behavior of two members of a three-person legislature always adversely affects the nontrader.[4] This is a feature of the system of majority voting, as we shall demonstrate below. Indeed, externalities may involve potential benefits to the nonparticipant. We shall present an extension of the paradox of vote trading to unanimity decision rules below in which externalities are indeed positive. Actually, the extension of the Riker-Brams paradox to unanimity rules is part of a broader theoretical question which we shall consider: under what conditions is a more stringent decision rule or a change in voting strategies Pareto optimal? Accordingly, after a consideration of the Riker-Brams argument, we shall extend their analysis and attempt a resolu-

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[1] See James M. Buchanan and Gordon Tullock, *The Calculus of Consent: Logical Foundations of Constitutional Democracy* (Ann Arbor: University of Michigan Press, 1962), chaps. 7–12; Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, 60 (June, 1970), 419–426; James S. Coleman, "The Possibility of a Social Welfare Function," *American Economic Review*, 56 (December, 1966), 1105–1122; Coleman, "The Possibility of a Social Welfare Function: Reply," *American Economic Review*, 57 (December, 1967), pp. 1311–1317; Edwin T. Haefele, "Coalitions, Minority Representation, and Vote-Trading Probabilities," *Public Choice*, 8 (Spring, 1970), 75–90; and Dennis C. Mueller, Geoffrey C. Philpotts, and Jaroslav Vanek, "The Social Gains from Exchanging Votes: A Simulation Approach," *Public Choice*, 13 (Fall, 1972), 55–80.

[2] Riker and Brams, "The Paradox of Vote Trading," *American Political Science Review*, 67 (December, 1973), 1235–1247. Cf. R. E. Park, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, 57 (December, 1967), 1300–1304; Mueller, "The Possibility of a Social Welfare Function; Comment," *Ibid.*, pp. 1304–1311; Robert Wilson, "An Axiomatic Model of Logrolling," *American Economic Review*, 59 (June, 1969), pp. 331–341; David H. Koehler, "Vote-Trading and the Voting Paradox: A Proof of Equivalence," *American Political Science Review*, 69 (1975); and, for a view which tends to support Riker and Brams but does not rule out the positions of Buchanan, Tullock and Coleman, see Peter Bernholz, "Logrolling, Arrow Paradox and Cyclical Majorities," *Public Choice*, 15 (Summer, 1973), 87–95 (but especially p. 94). But cf. the exchange among Tullock, Bernholz, and Riker and Brams in "Communications," *American Political Science Review*, 68 (December, 1974), 1688–1692. Also see Joe A. Oppenheimer, "Relating Coalitions of Minorities to the Voters' Paradox, or Putting the Fly in the Democratic Pie," University of Texas mimeo, 1973. An earlier informal statement of the possible negative effect of vote trading is found in William A. Niskanen, Jr., *Bureaucracy and Representative Government* (Chicago: Aldine, 1971), p. 143.

[3] Riker and Brams, "The Paradox of Vote Trading," p. 1240.

[4] *Ibid.*, p. 1242.

**Table 1. Utilities of Members for Outcomes of Voting**

| | Preferred Outcome | Motion $x$ $u_i(X)$ | $u_i(\tilde{X})$ | Preferred Outcome | Motion $y$ $u_i(Y)$ | $u_i(\tilde{Y})$ |
|---|---|---|---|---|---|---|
| | Col 1 | Col 2 | Col 3 | Col 4 | Col 5 | Col 6 |
| *Member 1 | $X$ | 1 | $-2$ | $Y$ | 1 | $-2$ |
| Member 2 | $X$ | 1 | $-1$ | $\tilde{Y}$ | $-2$ | 2 |
| Member 3 | $\tilde{X}$ | $-2$ | 2 | $Y$ | 1 | $-1$ |
| | | Motion $w$ $u_i(W)$ | $u_i(\tilde{W})$ | | Motion $z$ $u_i(Z)$ | $u_i(\tilde{Z})$ |
| Member 1 | $W$ | 1 | $-1$ | $\tilde{Z}$ | $-2$ | 2 |
| Member 2 | $\tilde{W}$ | $-2$ | 2 | $Z$ | 1 | $-1$ |
| *Member 3 | $W$ | 1 | $-2$ | $Z$ | 1 | $-2$ |
| | | Motion $t$ $u_i(T)$ | $u_i(\tilde{T})$ | | Motion $v$ $u_i(V)$ | $u_i(\tilde{V})$ |
| Member 1 | $\tilde{T}$ | $-2$ | 2 | $V$ | 1 | $-1$ |
| *Member 2 | $T$ | 1 | $-2$ | $V$ | 1 | $-2$ |
| Member 3 | $T$ | 1 | $-1$ | $\tilde{V}$ | $-2$ | 2 |

* Nontrader on pair of motions.

tion on the supposed effects of vote trading between the Buchanan-Tullock-Coleman approach and that of Riker and Brams.

The paradox of vote trading under a majority decision rule is based upon a simple set of criteria for "naive" or "sincere" voting (always revealing one's actual preferences) and "sophisticated" voting (concealing actual preferences in a way which nevertheless will yield a net gain in expected utility).[5] The Riker-Brams model stipulates two conditions for "switching" (or, sophisticated voting): (1) a switcher must be in the decisive set for a given motion; and (2) he must also be a pivotal member of such a set—in the sense that his change in votes would be sufficient to make a formerly losing coalition a winning one.[6] The second condition contains the further premise that there is some positive utility in trading of votes, so that an incentive to employ sophisticated rather than sincere voting does exist. Vote trading is simply "a barter system of switching" in which each legislator who argees to trade his vote on one motion receives the vote of another member in return.[7] What Riker and Brams show, then, is that in an $n$-member legislature voting on a set of motions, trades which appear to be individually rational among each pair of voters are not collectively rational (i.e., Pareto optimal) because of the effects of externalities on the nontraders. An example drawn from the Riker-Brams article will demonstrate the paradox.

In Table 1, the utilities for each legislator on each motion which Riker and Brams stipulate are reproduced.[8] In the table we have six motions: $x$, $y$, $w$, $z$, $t$, and $v$ and twelve possible outcomes: $X$, $\tilde{X}$, $Y$, $\tilde{Y}$, $W$, $\tilde{W}$, $Z$, $\tilde{Z}$, $T$, $\tilde{T}$, $V$, and $\tilde{V}$, where $u_i(X)$ is the utility to voter $i$ of the passage of motion $x$ and $u_i(\tilde{X})$ is the utility to $i$ for the defeat of the motion. Utility is defined over outcomes, since the motions are merely means to the end of maximizing total utility for each individual. It is clear from the table that under simple majority rule without vote trading, the net utilities over the six motions are zero for each player.[9] While all six motions pass, variations in the intensity of preferences affect the net utility such that no one is any better or worse off than had no votes been cast at all. If each legislator ignores the actions of others on votes on which he himself is not in a position to trade, logrolling appears to be a rational strategy. The net expected utility of the six votes is $+4$ for each member, allowing each to make the best possible trades for himself.[10] If, on the other hand, Member 1 resigns himself to his "security level"—his evaluation of what happens to him if he refrains from trading but others do not (what he can guarantee himself regardless of the actions of other players)—his net utility would be $-6$.[11] In the former case, the member considers only the possible gains from vote trading, and in the latter case, only what happens to

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[5] On the distinction between "sincere" and "sophisticated" voting, see Robin Farquharson, *Theory of Voting* (New Haven: Yale University Press, 1969), pp. 18 and 39–40.

[6] Riker and Brams, "The Paradox of Vote Trading," pp. 1237–1238.

[7] *Ibid.*, p. 1238.

[8] This is Table 5, p. 1241.

[9] *Ibid.*, Table 6.

[10] *Ibid.*, Table 7.

[11] *Ibid.*, Table 9.

**Table 2. Utilities of Outcomes Without Trades Under a Unanimity Rule**

| | Outcomes | | | | | | |
|---|---|---|---|---|---|---|---|
| Members | $u_i(X)$ | $u_i(Y)$ | $u_i(W)$ | $u_i(Z)$ | $u_i(T)$ | $u_i(V)$ | $\Sigma u_i$ |
| 1 | −2 | −2 | −1 | 2 | 2 | −1 | −2 |
| 2 | −1 | 2 | 2 | −1 | −2 | −2 | −2 |
| 3 | 2 | −1 | −2 | −2 | −1 | 2 | −2 |

him if he refuses to trade while the other players do trade. An evaluation of the potential gains from trading which excludes external costs convinces each member that all individual trades are rational. Thus, there is an incentive for every possible trade to occur. As a result of the logrolling, all six motions in Table 1 fail, yielding a net utility to each player of −2. This is the "paradox of vote trading." Each member fails to consider the external costs on the motions on which he does not trade and accepts any trade which appears to be individually rational. Rational trading thus yields a worse net situation than the failure to trade. Since no single member can be guaranteed that the others would agree not to trade if he refrains from doing so, each is put in a "kind of $n$-person Prisoners' Dilemma in which each member is forced by individual rationality to choose the socially and individually destructive alternative" and ". . . each member comes to a worse end than if he . . . always voted sincerely or naively."[12]

Riker and Brams have indeed revealed a paradox of rationality—one which they claim is "inescapable."[13] Advocates of the strategy of logrolling to maximize total utility—including Buchanan, Tullock, and Coleman—thus seem to be faced with a direct challenge to their position. In the next section of this paper, we shall extend the work of Riker and Brams to unanimity voting rules, providing further support for their finding of a paradox of vote trading.

### Unanimity Decision Rules and the Paradox

The occurrence of the paradox of vote trading under simple majority rule is not sufficient to warrant the statement of Riker and Brams that the paradox is inescapable. As Buchanan and Tullock have argued, simple majority rule is not well suited to handle the problems posed by varying intensities of preference. An examination of Table 1 clearly reveals that the members of the legislative body are not equally affected by the outcomes on each motion. We assume that utilities are measured cardinally for each individual.[14] In this sense, our approach differs from that of Riker and Brams, who argue that only ordinal measurement of utilities is required to establish the paradox.[15] We shall justify this approach in the next section. For the present purposes, we need to note only that for each member, the net difference in utility which would result from the passage or failure of any given motion is maximized for those motions on which the member is in the minority position. Under such conditions of varying intensities of preference, Buchanan and Tullock argue the case for a system of qualified majority rule and go so far as to suggest that the optimal decision rule might be one of unanimity, allowing for vote trading.[16] On motion $x$ for example, Members 1 and 2 each stand to gain one unit of utility if the motion passes, while Member 3 loses two units of utility with the same result. We might suspect that a more stringent voting rule would increase the net utility accruing to the three players more than would simple majority rule, either with or without vote trading.

The arguments of Buchanan and Tullock are, however, just as susceptible to the paradox of vote trading as is simple majority rule, although for quite different reasons. Under a unanimity rule without logrolling, the net utility over the six motions in Table 1 to each member is −2, with all motions failing. This result (see Table 2 below) is no better than majority rule with vote trading and inferior to majority rule without trading. If vote trading is allowed under unanimity, the net utility to each player is once again zero. (See Table 3). As in the case of simple majority rule, all six motions pass. Therefore, a unanimity rule allowing for vote trading is no better than simple majority rule with a prohibition on logrolling.[17] Furthermore, an examination of the effects of the alternative strategies of naive and sophisticated voting on the unanimity rule and utility values in Table 1 indicates that we have another occurrence of the paradox of vote trading. In the Riker-Brams example for majority rule, the individual trades were rational, but the aggregation of the

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[12] *Ibid.*, pp. 1244 and 1242.

[13] *Ibid.*, p. 1242.

[14] This amounts to accepting interpersonal comparisons of utilities. See Jerome Rothenberg, *The Measurement of Social Welfare* (Englewood Cliffs, N.J.: Prentice-Hall, 1961), pp. 137–143.

[15] Riker and Brams, "The Paradox of Vote Trading," pp. 1242 ff.

[16] Buchanan and Tullock, *The Calculus of Consent*, pp. 85ff. and pp. 276–280.

[17] For a contrary point of view, see J. Roland Pennock, "The Pork Barrel and Majority Rule," Journal of Politics, 32 (August, 1970), 709–716.

**Table 3. Utilities of Outcomes with Trades and with External Costs Under a Unanimity Rule**

| | Outcomes | | | | | | |
|---|---|---|---|---|---|---|---|
| Members | $u_i(X)$ | $u_i(Y)$ | $u_i(W)$ | $u_i(Z)$ | $u_i(T)$ | $u_i(V)$ | $\Sigma u_i$ |
| 1 | 1 | 1 | 1 | −2 | −2 | 1 | 0 |
| 2 | 1 | −2 | −2 | 1 | 1 | 1 | 0 |
| 3 | −2 | 1 | 1 | 1 | 1 | −2 | 0 |

**Table 4. Utilities of Outcomes with Trades When Possible and Without Anticipating External Costs Under a Unanimity Rule**

| | Outcomes | | | | | | |
|---|---|---|---|---|---|---|---|
| Members | $u_i(X)$ | $u_i(Y)$ | $u_i(W)$ | $u_i(Z)$ | $u_i(T)$ | $u_i(V)$ | $\Sigma u_i$ |
| 1 | −2 | −2 | 1 | −2 | −2 | 1 | −6 |
| 2 | 1 | −2 | −2 | 1 | −2 | −2 | −6 |
| 3 | −2 | 1 | −2 | −2 | 1 | −2 | −6 |

trading outcomes produced a sub-optimal societal result. For a unanimity rule, the trading of votes would produce a Pareto optimal societal (and, of course, individual) result, but, as we shall see, the individual trades are not themselves rational.

In the Riker-Brams example, the utility of the outcome to each legislator who trades but does not consider external costs is +4. For a unanimity decision rule, however, the failure to consider the impact of the trading behavior of others yields a utility of −6 to each member across the six motions (see Table 4). Thus, if each legislator considered only those trades in which he takes an active part, he would rationally refuse to trade. Consider the case of a potential exchange between members 2 and 3 on motions *x* and *y*. The only feasible trade would be for member 2 to give up his decisive opposition to motion *y* in return for member 3's pivotal support on *x*. In each case, however, the result would be a loss in utility. Voting sincerely across the two motions, each member would receive +1 units of utility: −1 on the defeated issue he supports and +2 on the defeated issue he opposes. An exchange of votes would yield each a net utility of −1: −2 on the passed motion he opposes and only +1 on the passed motion which he supports. It would appear, then, that there are no reasons why members should trade votes at all.

Such an appearance, however, is misleading because of the altered effects of externalities. Under a more exclusive voting rule, as Buchanan and Tullock correctly argue, the costs imposed bv externalities indeed will be reduced.[18] In the case of the current voting situation, logrolling will produce external benefits rather than costs for the traders. If each legislator were to refuse to trade and attempt to "await" the benefits accruing to him from the trading behavior of the other two legislators, he would receive (if such a situation were actually to occur) +8 units of utility—in comparison with the Riker-Brams security level of −6 for majority rule (see Table 5). While such trades will not take place—since they are individually irrational—the analogy with the Riker-Brams security level is instructive since it demonstrates that logrolling under a unanimity rule has a very different effect upon externalities than does vote trading under majority rule. This ideal situa-

**Table 5. Utilities of Outcomes When No Trades Occur But Each Member Anticipates External Costs Under a Unanimity Rule**

| | Outcomes | | | | | | |
|---|---|---|---|---|---|---|---|
| Members | $u_i(X)$ | $u_i(Y)$ | $u_i(W)$ | $u_i(Z)$ | $u_i(T)$ | $u_i(V)$ | $\Sigma u_i$ |
| 1 | 1 | 1 | 1 | 2 | 2 | 1 | 8 |
| 2 | 1 | 2 | 2 | 1 | 1 | 1 | 8 |
| 3 | 2 | 1 | 1 | 1 | 1 | 2 | 8 |

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[18] Buchanan and Tullock, *The Calculus of Consent*, pp. 63–68.

**Table 6. The Paradox of Vote Trading as a Three-Person Prisoners' Dilemma**

| Member 3 | Alternative 1 Not Trade | Member 2 | |
|---|---|---|---|
| | | Alternative 1 Not Trade | Alternative 2 Trade |
| Member 1 | Alternative 1 Not Trade | −2, −2, −2 | −2, −2, −2 |
| | Alternative 2 Trade | −2, −2, −2 | −4, −4, 8 |
| Member 3 | Alternative 2 Trade | Member 2 | |
| | | Alternative 1 Not Trade | Alternative 2 Trade |
| Member 1 | Alternative 1 Not Trade | −2, −2, −2 | 8, −4, −4 |
| | Alternative 2 Trade | −4, 8, −4 | 0, 0, 0 |

Note: This is an adaptation of Table 10 in Riker and Brams, "The Paradox of Vote Trading," p. 1243. In cell (1, 1, 1), the entry is "−2, −2, −2" because no one trades. In cells (1, 2, 1), (2, 1, 1) and (1, 1, 2), the entries are also "−2, −2, −2" because, if any pair consistently votes sincerely, then all three must do so. In cell (2, 2, 2), the entry is "0, 0, 0" because all players trade. In cells (2, 2, 1), (1, 2, 2) and (2, 1, 2), the entries are appropriate combinations of −4 units for the traders and +8 units for the nontraders. The +8 units are calculated as in Table 5; the −4 units result from receiving, first the payoff to sincere voting (a) when one is not a trader (−2−2) and (b) when one's potential trade is with a sincere voter (1−2); and second, the payoff to *sophisticated voting when one's partner is a sophisticated voter (2−1). If trading is viewed as "cooperative"* behavior and not trading as "defecting," then the desired outcome in a Prisoners' Dilemma game—total cooperation—is represented in cell (2, 2, 2); this strategy is preferred by all players to complete defection—as represented in cell (1, 1, 1)—thus producing the dilemma.

tion which each player would like to obtain is thus simply the sum of all of the positive utilities for the six motions for each legislator: each member can guarantee himself a utility of +2 on those motions he opposes by refusing to trade; the trading patterns on the motions he favors guarantee that an outcome favorable to him will occur.

We thus have the reverse of the kind of *n*-person Prisoners' Dilemma found by Riker and Brams: each player would be better off if he could refuse to trade himself but would benefit from the external effects of the trades of the other players. Each member has an incentive to be a free rider on the others. By his single negative vote on a given motion he opposes, he can guarantee himself a utility of +2, while no trades will be consummated which will reject the motions he favors. Since, by construction, the nontrading member in Table 1 favors the motion on which he does not trade, he cannot incur any losses on such votes. Yet, if Members 1 and 3 were to realize that Member 2 did not intend to cooperate on other motions, they would rationally refuse to trade at all. The resulting paradox is that no players would ever trade, leaving each member worse off than he would be if the (individually irrational) trades were consummated. Table 6 presents the Prisoners' Dilemma for a unanimity rule.[19] The paradox does indeed seem "inescapable" and the entire situation is symmetric. The net utility to each legislator under simple majority rule without vote trading is equal to the net utility under a unanimity rule with vote trading; while the net utility obtained under both majority rule with vote trading and unanimity without logrolling is identical. These findings make more explicit a statement by E. E. Schattschneider about the bargaining system in American politics: ". . . if everybody got into the act, the unique advantage of this form of organization would be destroyed. . . ."[20]

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[19] Riker and Brams (see the note to Table 10 on p. 1243 of "The Paradox of Vote Trading") argue that their result is "strictly a Prisoners' Dilemma. . . ." If, however, we consider the general discussion of the dilemma, the preferred strategy of both players is cooperating (i.e., trading), but such a strategy is dominated by that of defecting (not trading). This is the case in our example; for a discussion of the dilemma, see R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957), pp. 88–113. The Riker-Brams example is indeed a Prisoners' Dilemma, but not the classical type.

As in any Prisoners' Dilemma game, the assumption of Riker and Brams that "motions are made and voted upon serially, so that future motions are not necessarily anticipated when current motions are considered"[21] does provide the potential for learning behavior over time. In repeated legislative sessions, members may begin to trust each other and pursue a strategy of cooperation rather than defection under a unanimity rule (as opposed to cooperation under majority rule) yielding the best possible outcome.[22] Yet the logical properties of the paradox remain intact and there is nothing to prevent a greedy member from attempting to maximize his own utility at the expense of the others, resulting again in a Prisoners' Dilemma and an occurrence of the paradox of vote trading. Thus, we have shown that the paradox is quite general, but that its effects are not neutral with respect to decision rules. Who bears the external costs depends upon who has the power to inflict them—the majority (in the case of majority rule) or the minority (under a unanimity rule).[23] On a more general level, however, we ask: Are the positions of Riker and Brams, on the one hand, and Buchanan, Tullock, and particularly Coleman on the other hand, necessarily in total conflict? It is to this question—and its implications for the relationship between vote trading and decision rules—that we now turn.

## Paradox Gained and Paradox Lost

The initial discussion of the paradox by Riker and Brams suggests that they have uncovered an anomaly in the structure of voting procedures: the initial assumption is that vote trading should produce Pareto optimal results, but the paradox is established when the condition of vote trading is not met. Riker and Brams argue, however:

> . . . the paradox of vote trading is closely related to the paradox of voting. . . . [In the above example] no trade is preferred to all other trades, which is equivalent to our earlier demonstration that no coalition can defeat all others. Since there is no preferred set of trades on which to base a stable coalition, no stable coalition is possible. The paradox of vote trading cannot, therefore, be simply solved by waving it away with a coalition. Rather it is inherent in the nature of the legislative process and, given an appropriate distribution of tastes and external costs, cannot be avoided.[24]

Similarly, David H. Koehler has presented a proof that a cyclical majority will occur if and only if the conditions for vote trading established by Riker and Brams and discussed above are met.[25] Note, however, the statement by Riker and Brams that "an appropriate distribution of tastes and external costs" must occur. Perhaps a reconciliation of the position espoused by Buchanan, Tullock, and Coleman and that of Riker and Brams and Koehler can be achieved. A critical first step in this process is to realize that the Riker-Brams discussion of the paradox of vote trading when preferences are ordinarily ranked is itself based upon an "appropriate" distribution of tastes and external costs. The salience measure they propose for their ordinal treatment of the paradox, $s_i(x)$, is itself a function of the associated cardinal utilities attached by an individual to the passage and failure of a given motion. Let $u_i(P_x)$ be the utility to Member *i* derived from the outcome associated with his preferred position on motion *x* and let $u_i(P_x)$ be a similar utility for *i* derived from the outcome for his not preferred position on *x*. Then the salience of the motion is defined as: $s_i(x) = |u_i(P_x) - u_i(\tilde{P}_x)|$, which is clearly a function of the cardinal properties of the associated utilities.[26]

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[20] E. E. Schattschneider, *The Semisovereign People* (New York: Holt, Rinehart and Winston, 1960), p. 35. The paradox—at least as developed so far—follows rather directly from the fact that each voter can be said to be in either a winning coalition (for each motion or for the entire set of motions) or a losing coalition. Thus, the set of motions, $J$, constitutes a simple game: formally, "[*a*] game, $v$, in (0, 1) normalization is said to be simple if, for each $S \subset N$, we have either $v(S) = 0$ or $v(S) = 1$" (Guillermo Owen, *Game Theory* [Philadelphia: W. B. Saunders, 1968], p. 163), where N is the set of players in the game, $S$ is a coalition, and $v(S)$ is the value of the coalition. Owen notes (*ibid.*) that "simple games . . . include voting 'games' in elections and legislatures"—precisely the situation we have. The result that at least one member of the total number of players must be excluded from some coalition follows from an extension of L. S. Shapley's comment that "[s]olutions that exclude one or more players . . . are found . . . in almost all simple games, and they probably exist in all constant-sum games." See Shapley, "On Solutions that Exclude One or More Players," in *Essays in Mathematical Economics in Honor of Oskar Morgenstern,* ed. Martin Shubik (Princeton: Princeton University Press, 1967), p. 57.

[21] Riker and Brams, "The Paradox of Vote Trading," p. 1236.

[22] Cf. Anatol Rapoport and Albert M. Chammah, *Prisoners' Dilemma* (Ann Arbor: University of Michigan Press, 1965), chap. 5; and Lester B. Lave, "An Empirical Approach to the Prisoner's Dilemma Game," *Quarterly Journal of Economics,* 76 (August, 1962), pp. 424–436. Nigel Howard has proposed an approach to the Prisoners' Dilemma in which cooperative strategies would be rational. In his "metagames," players choose their strategies dependent upon those of the other players. See his *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (Cambridge: MIT Press, 1971), pp. 44–48 and 55–60. Note the strong criticism of this approach by John C. Harsany, in his review of Howard's book in the *American Political Science Review,* 67 (June 1973), 599–600. In experiments with repetitive play, individuals have tended to behave cooperatively. Rapoport and Chammah as well as Lave found that the proportion of cooperative responses approach seventy per cent. For an interesting argument that the Prisoners' Dilemma is not a genuine dilemma or even a paradox, see R. L. Cunningham, "Ethics and Game Theory: The Prisoners' Dilemma," *Papers on Non-Market Decision Making,* 2 (1967) pp. 11–26.

[23] The logical nexus between unanimity and majority voting rules vanishes once the deterministic nature of the argument is relaxed. The probability that an individual will agree to trade votes with one or more others is dramatically affected by the fact that, under the former decision rule, a single negative vote will suffice to defeat a motion. Under majority rule, however, the probability that a member will be able to affect the outcome of a collective decision is substantially reduced—and, indeed, is a function of the size of the voting body. Another factor which is also a function of the size of the voting body—and which seems to us to be more serious for a unanimity than a majority rule—is "transaction costs," associated with the necessity of reaching a collective decision. Our discussion of pairwise vs. n-wise trading of votes below bears on this problem. We are grateful to T. Nicolaus Tideman and James M. Buchanan for calling this general problem to our attention, although we emphasize that our approach is not always consistent with theirs (in private communications). Cf. also note 34 below.

[24] Riker and Brams, "The Paradox of Vote Trading," pp. 1245–1246.

Alterations in these cardinal utilities thus *may* entail alterations in the relative salience of the various motions, with the prospect that an "appropriate" distribution of tastes is not obtained. To take two examples,[27] in Table 1 above (a) consider the impact of changing the values of each motion on which the cardinal utilities are currently $(1, -2)$ to $(2, -1)$; or (b) consider a change from each entry of $(-2, 2)$ to $(-3, 3)$. Under the first change, a majority voting rule would yield each member a net utility of zero without vote trading and $+2$ if all players traded votes. The second proposed alteration of utilities in Table 1 would produce a unanimity rule outcome of $-2$ to each member without vote trading and of zero with logrolling. In both cases, the paradox is seemingly avoided. How can this occur?

The reason the paradox might be avoided under certain circumstances is that the distribution of preferences has a direct and identifiable impact on the vote trading situation. A second factor which must not be overlooked is that the ordinal properties of the paradox of voting can be maintained even if there are changes in the cardinal numbers associated with the specific outcomes in a chart of preferences such as Table 1. The change from utilities of $(-2, 2)$ to $(-3, 3)$ does not affect the ordinal rankings of the outcomes at all with respect to producing a paradox of voting. Coleman's critical point is that there is a difference between merely noting that one is in a situation in which the paradox of voting occurs and attempting to devise a strategy to extricate oneself from such a situation.[28] On the other hand, we must realize that at a fundamental level the paradox of vote trading is indeed inescapable. As long as individual trades are rational, then we have a cyclical pattern of vote trading which can never be stable, as the Riker-Brams analysis demonstrates.[29] This result is *not* a function of the associated cardinal utilities which members attach to outcomes. It thus should be cautioned that we are not proposing a logical way out of the paradox (although a very restrictive one will follow from a theorem we propose), but rather a practical way out. In particular, we argue that there are basically three types of situations: those in which sincere voting is Pareto optimal, those in which vote trading (sophisticated voting) is Pareto optimal, and those in which no decision rule and no voting strategy can improve upon a preordained societal result. In any of these situations, strategies which yield less than an optimum may indeed be reached. Our concern, however, is to distinguish the conditions under which the paradox of vote trading will occur and those under which vote trading will yield a Pareto optimal solution. In order to make this distinction, we need a set of cardinal utility measures.

We thus propose such a set: $u(x_{ij})$ for a set of voters $i \in I$ and a set of motions $j \in J$ where $u(x_{ij})$ is the utility of the passage of the *j*th motion to the *i*th voter: and $u(\tilde{x}_{ij})$ where the various subscripts are as before and $u(\tilde{x}_{ij})$ is the utility of the defeat of such motions. We offer the following theorem to provide necessary and sufficient conditions for the optimality of vote trading:

*Theorem*

(1) Sincere voting is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\tilde{x}_{ij}).$$

(2) The strategy adopted (sincere vs. sophisticated voting) is neutral under majority rule if and only if:[30]

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[25] Koehler, "Vote-Trading and the Voting Paradox." See Appendix II.

[26] Riker and Brams, "The Paradox of Vote Trading," p. 1239.

[27] We are grateful to David H. Koehler (private communication) for suggesting the first example and to an anonymous referee for suggesting the second.

[28] Coleman, "The Possibility of a Social Welfare Function," especially p. 1107.

[29] Riker and Brams, "The Paradox of Vote Trading," p. 1244.

[30] Cf. Buchanan and Tullock, *The Calculus of Consent* (p. 145): "Potentially, the voter should enter into bargains until the marginal 'cost' of voting for something of which he disapproves but about which his feelings are weak exactly matches the expected marginal benefits of the vote or votes secured in return support for issues in which he is more interested."

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}).$$

(3) Vote trading is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij}).$$

In the first appendix to this article, we present the proofs of the above optimality conditions. As formulated above, these proofs depend upon the assumption either that all motions pass or that all motions fail. We shall continue to employ this assumption in our discussion of the optimality conditions below because of its interesting consequences for "porkbarrel" projects on which legislators may trade votes. This restriction, however, can easily be lifted and our results generalized to situations in which some motions pass and others fail. What is critical here is that the relationships we derive are based upon the positions taken by the majority of legislators, on the one hand, and the minority of legislators, on the other hand.[31] Note that in part (1) of the theorem, Pareto optimality is itself defined by

$$\sum_i \sum_j u(x_{ij})$$

since this summation provides the boundaries for the production possibility curve in terms of the total utility for the society. Similarly, Pareto optimality for condition (2) can be represented by either

$$\sum_i \sum_j u(x_{ij}) \quad \text{or} \quad \sum_i \sum_j u(\bar{x}_{ij}),$$

while for condition (3), the optimality condition obviously is represented by

$$\sum_i \sum_j u(\bar{x}_{ij}).$$

We note that in the theorem, we do not restrict the trading situation to pairwise trades, as do Riker and Brams. The set of trading partners can be as large as the entire membership of the legislative body. This lack of restriction to pairwise trades will have interesting consequences for analyzing the paradox of vote trading under different situations of Pareto optimality.

We state a corollary of the theorem without proof: Under condition (1) vote trading is Pareto optimal under a unanimity rule; under condition (2) the strategy adopted under a unanimity rule is neutral; and under condition (3) sincere voting is Pareto optimal under a unanimity rule. The last part of the corollary is perhaps the least obvious. In our discussion above, we noted that the effects of vote trading under a unanimity rule are indeed just the opposite of those under majority rule. Therefore, if vote trading is optimal under majority rule for condition (3), sincere voting would be expected to be (and indeed is) optimal under a unanimity rule.

## Conclusions

The appropriate distribution of tastes and externalities for the optimality of sincere voting, then, is simply that the voters collectively have more to gain by the passage of all the motions than they do by defeating them. This result is heuristic: majority rule without vote trading will produce a Pareto optimal result under such conditions. If the voters attach equal utility across the set of motions to their passage or their defeat, then no decision rule or strategic premise is going to change the net utility of the outcome. Finally, if in the aggregate, the voters attach more utility to the defeat of all motions than they do to passing all of them, then majority rule with vote trading (or a unanimity rule without logrolling) can yield a Pareto optimal result. Only, however, in the very restrictive case in which the size of the trading set is equal to the number of legislators will the paradox of vote trading be avoided in a formal sense. In this situation, we have a totally cooperative $n$-person game in which the enforcement power has been agreed upon by the players in advance. More likely situations involve either pairwise trades only—a noncooperative game—as developed by Riker and Brams and also by Gerald H. Kramer, or at best only a partially cooperative game (in which trading coalitions may be greater than pairwise in size but smaller than the entire chamber), as John A. Ferejohn has argued.[32] Thus, the optimality of vote trading under condition (3) does not extricate us from the paradox of vote trading unless we make the very strong assumption that the trading game is totally cooperative. On the other hand, all is not lost because *in actual legislative situations*, we would expect that "learning behavior" could easily provide the basis for legislators to extricate themselves from the paradox of vote trading. And, indeed, to the extent that legislators are rational actors, we should expect that they will indeed do so.

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[31] This condition together with the condition for vote trading introduced in the proofs below, is based upon situations in which (under majority rule) all motions will pass. While seemingly restrictive, the same logic underlying the three proofs can easily be extended to "corollaries" assuming that all motions will fail or that some will pass and some will fail. A less cumbersome approach, however, can extend the theorem we have presented rather easily. Let $u(x_{ij})$ represent the utility to legislator $i$ of the *majority* position on issue $j$ and $u(\bar{x}_{ij})$ represent the utility to $i$ of the *minority* position on $j$ (irrespective of the nature of the actual outcomes). Then the theorem we propose is completely general and subsumes not only our special case but all of the possible "corollaries" representing alternative cases.

[32] See Riker and Brams, "The Paradox of Vote Trading," pp. 1238ff.; Kramer, "Sophisticated Voting over Multidimensional Choice Spaces," *Journal of Mathematical Sociology*, 2 (July, 1972), 125–180; and Ferejohn, "Sour Notes on the Theory of Vote Trading," California Institute of Technology, Social Science Working Paper Number 41 (June 1, 1974). We are particularly grateful to Steven J. Brams for his comments on this aspect of the present work.

It is not terribly difficult for a rational legislator to realize when to stop trading and when the Pareto optimal position has been reached (particularly if he has been "stung" once by an occurrence of the paradox of vote trading). While Riker and Peter C. Ordeshook have argued that in most large legislatures members are likely to realize the potentially harmful effects of vote trading (and, hence, restrict any trading activity to a smaller protocoalition such as fellow party members),[33] evidence from the American Congress suggests that "learning behavior" is indeed evident on the optimality of vote trading on "porkbarrel" votes. As James T. Murphy has commented:

> [On the House Public Works Committee] . . . the two parties *distrust* each other. After all, public works projects are inherently political and, as such, are presumed to have substantial electoral significance. Since each party values majority status, each expects the other to porkbarrel . . . . when the other is in the majority. Because they foresee the possibility of partisan allocations and because they take the allocation of public works benefits so seriously, congressmen insist on a fair allocation of the goodies. Hence, they have agreed upon permanent ways to reduce electoral risks when a majority in each party is affected by an allocation. . . . . *Only when a majority in each party benefits from an allocation can party cooperation be expected.* Since committee Democrats and Republicans are confronted with exactly the same situation as House Democrats and Republicans, the electoral risk-reducing devices explain party cooperation in the committee as well as in the House.[34]

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[33] Riker and Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), pp. 113–114. Note especially their reference to James Murphy, *The Empty Porkbarrel* (Lexington, Mass.: D. C. Heath, forthcoming), which (according to Riker and Brams) argues that very little logrolling actualy takes place even on public works legislation. Also, cf. David R. Mayhew, *Party Loyalty among Congressmen* (Cambridge: Harvard University Press, 1966: ch. 6) on the optimality of intra-party as opposed to inter-play cooperation. And, cf. the comments made in President Gerald R. Ford's inaugural speech to a joint session of Congress on August 12, 1974. Addressing in particular House Speaker Carl Albert (D., Okla.), Ford stated: "I have sometimes voted to spend more taxpayers' money for worthy federal projects in Grand Rapids [Michigan] while vigorously opposing wasteful federal boondoggles in Oklahoma" [cited in *Congressional Quarterly Weekly Report*, 32 (August 17, 1974), 2209] Also cf. the comments of Martin and Susan Tolchin (*To the Victor* [New York: Vintage, 1972], p. 218): ". . . it was no surprise that when Representative Charles Joelson, a New Jersey Democrat, wrote a letter to each member of Congress asking 'Where can we economize in your district?' not one reply was returned to his office." Many members probably thought that the letter was just another example of the humor for which Joelson was known (and to which the first-named author of this article can attest). Further evidence for the claim that legislators *may* be aware of the paradox of vote trading can be inferred from Herbert B. Asher, "The Learning of Legislative Norms," *American Political Science Review*, 67 (June, 1973), 499–513. Asher notes (p. 508) that 68 per cent of a sample of 22 freshmen in the 91st House stated that they would be willing to trade votes with a colleague. While the effects of learning behavior do seem to be apparent in the House—more nonfreshmen (81 per cent of a sample of 21) expressed a willingness to trade than freshmen (p. 501)—the comment of a senior member is instructive (p. 503): "Yes, I'd trade votes, but this does not happen often. It is not a specific trade, but more a matter of good will. I never had a specific trade; this happens more often in the Senate." (On the utility of trading through "good will," or implicit logrolling, see Appendix II.)

[34] Murphy, "Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision-Making," *American Political Science Review*, 68 (March 1974), 179–180 (first emphasis in original; second emphasis added). We thus find ourselves in the interesting situation of citing a single author on two different sides of the same question (cf. note 33 above). We await publication of his book before pursuing the interpretation of Riker and Ordeshook any further. For similar comments, see Robert F. Fenno, Jr., *Congressmen in Committees* (Boston: Little, Brown, 1973), pp. 58, 156–159, and 165–166; and Mayhew, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1974), pp. 86–91. For a somewhat different perspective on the effects of vote trading for porkbarrel projects from that presented in the text, see Ferejohn, "Sour Notes on the Theory of Vote Trading," p. 11. For a statement which is more in accord with our view of the logrolling problem and also treats the problem of transaction costs (cf. note 23 above), see Mancur Olson, Jr., "The Optimal Allocation of Jurisdictional Responsibility: The Principle of 'Fiscal Equivalence'," in United States Congress, Joint Economic Committee, 91st Congress, First Session, *The Analysis and Evaluation of Public Expenditures: The PPB System*, I (Washington: Government Printing Office, 1969), pp. 321–331. Olson argues (p. 326): "If all mutually advantageous bargains were struck, logrolling would insure that all collective goods that it was Pareto-optimal to provide would be provided. But . . . especially where large groups of people are at issue, it will very often be the case that logrolling will not happen, and that there will not be a Pareto-optimal supply of public goods." He adds (p. 326, n. 14): "In the United States Congress, logrolling probably leads to a greater expenditure when projects of a 'pork barrel' type are at issue. In most of these cases the projects are of a tangible, if not monumental type, and a Congressman is more likely to be identified in his district with such a project than with a general tax increase, which could not in any case usually be traced to any one package of local projects." For an argument similar to that of Olson on government expenditures, see J. Ronnie Davis and Charles W. Meyer, "Budget Size in a Democracy," *Southern Economic Journal*, 36 (July, 1969), 10–17.

Thus, on issues in which individual rationality might lead a member to vote against a porkbarrel project for another member (and, particularly, one from the opposition party), there still might be a collective rationality in voting for the project —and members seem to be able to "learn" how to resolve the formal problem of pairwise trading instability.

The result is traceable to the fact that straightforward majority rule nevertheless will produce majorities for all six motions under condition (3); this voting procedure is insensitive to variations in individual cardinal utilities. The conditions for the optimality of vote trading, however, are built around assumptions about cardinal utilities. When the decision reached by counting votes is the same as that reached by counting units of utility—as in condition (1)—there is no conflict between the majority principle and the salience of individual issues. On the other hand, when the two criteria come into direct conflict, the mechanism of vote trading (or of a unanimity rule without vote trading, which also is constructed so as to protect "intense minorities") has the potential to avoid a paradox of vote trading.

In particular, we are intrigued by the failure of a unanimity rule with vote trading to dominate the results obtained for such a rule without logrolling. In the first case, where a supposed domination does occur, we encounter the paradox of vote trading. However, the advocates of a unanimity rule with vote trading—Buchanan and Tullock—were particularly concerned with situations in which an intense minority might be tyrannized by a relatively apathetic majority. It is precisely in this situation—where

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij})$$

that vote trading is Pareto optimal under majority rule but inferior to sincere voting for a unanimity rule. The paradox with which we are confronted, then, is over the effects of alternative decision rules and voting strategies on the problem of externalities. We do not have a unidirectional linkage between the rules and the strategies, but a more complicated web than we had imagined. Indeed, the only situation in which a unanimity decision rule with vote trading is Pareto optimal involves "intense majorities" rather than "intense minorities."

The relationship between the operative decision rule and the optimal voting strategy (if such a strategy exists) is itself a function of the distribution of preferences—and thereby affects the distribution of externalities. As Riker and Brams comment, the critical distinction is between the "market for votes" and the "market for goods": vote trading is an imperfect barter system in which only whole commodities (votes) rather than infinitely divisible ones (e.g., money) must be exchanged.[35] This feature of the imperfect vote market increases the effects of both the decision rule and the voting strategy on the magnitude of external costs and benefits. Only when externalities are effectively nil

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij})$$

does the relationship between voting strategies and decision rules cease to affect the total utility over the set of outcomes.

In the case of the paradox of vote trading, the imperfections of the market produce a dynamic relationship between decision rules and voting strategies which effectively prevents the attainment of Pareto optimality. Under the final set of conditions

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij}).$$

either majority rule with vote trading or a unanimity rule without logrolling can yield a Pareto optimal outcome. Thus, the two mechanisms suggested by Buchanan and Tullock both may work to reduce external costs, but not simultaneously. When a unanimity rule yields an optimal distribution of utilities, vote trading can at best fail to improve upon this distribution and at worst product a markedly suboptimal distribution of external costs, thereby leaving everyone worse off than if no votes had been cast at all.

Thus our analysis has demonstrated that the positions of Riker and Brams, on the one hand, and that of Buchanan, Tullock, and Coleman, on the other hand, are not necessarily in total conflict (although for rather different reasons from those Buchanan and Tullock posited). Except in the extreme case in which *all* legislators agree to trade (for majority rule), the paradox of vote trading cannot be avoided as a logical problem. This does not mean, however, that sincere voting is Pareto optimal under all circumstances. What we emphasize here is that Riker and Brams are concerned with a formal problem of voting games, whereas Buchanan, Tullock, and Coleman have stressed optimality conditions. Clearly, as we have demonstrated, the two situations are not logically equivalent. Which solution is more satisfactory depends upon the dynamics of an actual social situation and the answer to the fundamental question: how high are the costs of reaching a collective decision (transactions costs) among all players?

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[35] Riker and Brams, "The Paradox of Vote Trading," pp. 1235 and 1238. Cf. Coleman, "The Possibility of a Social Welfare Function: Reply," pp. 1315–1317.

## Appendix I

In this appendix, we present a proof of the theorem (but not the corollary) stated in the text. We restate the definitions of $u(x_{ij})$ as the utility that the $i$th legislator receives from the passage of the $j$th motion and $u(\bar{x}_{ij})$ as the utility he receives from the defeat of such motions. Denote the total set of voters (on any motion) as I and partition the set I into the following disjoint subsets: A, the set of traders (on any motions) and B, the set of nontraders on all motions. We also restate the theorem:

*Theorem*

(1) Sincere voting is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\bar{x}_{ij}),$$

(2) The strategy adopted (sincere vs. sophisticated voting) is neutral under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}).$$

(3) Vote trading is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij}).$$

We shall also assume, without loss of generality, that

$$\sum_i \sum_j u(x_{ij}) = 0.$$

Given the definitions of Pareto optimality in the text, we immediately turn to the proof, which like the theorem itself, is in three parts.

First consider condition (1):

Let

$$\sum_i \sum_j u(x_{ij}) = 0 \text{ and } \sum_i \sum_j u(\bar{x}_{ij}) < 0$$

For trades to occur, we have:

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0.$$

For nontraders, by stipulation:

$$\sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

Since the total utility to all players is assumed to be zero,

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) + \sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

To prove sufficiency, let

$$\sum_i \sum_j u(\bar{x}_{ij}) < \sum_i \sum_j u(x_{ij}) = 0.$$

If all members trade votes (i.e., $B$ is empty):

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij}).$$

But this contradicts the initial premise that

$$\sum_i \sum_j u(\bar{x}_{ij}) < \sum_i \sum_j u(x_{ij})$$

since, if $B$ is empty

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0$$

$$\forall i \in A \rightarrow \sum_i \sum_j u(\bar{x}_{ij}) > 0.$$

But

$$\sum_i \sum_j u(\bar{x}_{ij}) < 0$$

by stipulation. Thus, $B$ is non-empty and sufficiency is established.

The proof of necessity is not as direct because

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\bar{x}_{ij})$$

does not follow directly from the fact that $B$ is non-empty. As condition (2) of the theorem demonstrates, the non-emptiness of $B$ is also consistent with

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}).$$

If B is non-empty, then either (1) vote trading yields a net loss of utility from sincere voting; or (2) vote trading among all $i \in I$ produces no net change of utility across the motions $j \in J$; or (3) only a subset of $I$, $i \in A$, can trade profitably and

$$\forall i \in B, \quad u(x_{ij}) < 0.$$

The condition for vote trading,

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0,$$

effectively rules out non-profitable trades among *all* $i \in I$, but it does not eliminate profitable trading among some subset of $A \subset I$. If such a subset can trade profitably, $B$ may be non-empty and

$$\sum_i \sum_j u(x_{ij}) \geq \sum_i \sum_j u(\bar{x}_{ij}).$$

The suboptimality of vote trading, however implies that:

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) < \sum_{i\in B}\sum_{j} u(x_{ij})$$

which in turn implies that $A$ is empty and $B$ is non-empty (specifically, the condition for vote trading is violated). Since

$$\sum_{i\in B}\sum_{j} u(x_{ij}) > \sum_{i\in A}\sum_{j} u(\bar{x}_{ij}),$$

$$\sum_{i\in B}\sum_{j} u(x_{ij}) > \sum_{i\in B}\sum_{j} u(\bar{x}_{ij}).$$

Otherwise, vote trading would be profitable $\forall i\in B$. Since

$$\forall i\in I,\quad i\in B,\quad \sum_{i\in B}\sum_{j} u(x_{ij}) > \sum_{i\in B}\sum_{j} u(\bar{x}_{ij})$$
$$\rightarrow \sum_{i}\sum_{j} u(x_{ij}) > \sum_{i}\sum_{j} u(\bar{x}_{ij}),$$

establishing necessity.

The proof for the second condition is as follows:

Let

$$\sum_{i}\sum_{j} u(x_{ij}) = \sum_{i}\sum_{j} u(\bar{x}_{ij}) = 0.$$

For trades to occur, we have:

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) > 0.$$

For the non-traders, by stipulation:

$$\sum_{i\in B}\sum_{j} u(x_{ij}) \leq 0$$

Thus

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) + \sum_{i\in B}\sum_{j} u(x_{ij}) \leq 0.$$

To prove sufficiency, let

$$\sum_{i}\sum_{j} u(x_{ij}) = \sum_{i}\sum_{j} u(\bar{x}_{ij}) = 0.$$

Assume that $B$ is empty. Then, by the condition for vote trading,

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) > \sum_{i}\sum_{j} u(x_{ij}) = 0$$
$$\rightarrow \sum_{i}\sum_{j} u(\bar{x}_{ij}) > \sum_{i}\sum_{j} u(x_{ij}).$$

since $\forall i\in I$, $i\in A$. But this yields a contradiction since

$$\sum_{i}\sum_{j} u(x_{ij}) = \sum_{i}\sum_{j} u(\bar{x}_{ij})$$

by assumption. Now assume that $A$ is empty. Then, by the condition for vote trading,

$$\sum_{i\in B}\sum_{j} u(x_{ij}) \leq 0.$$

If the strict inequality holds,

$$\sum_{i\in B}\sum_{j} u(x_{ij}) < 0 \rightarrow \sum_{i}\sum_{j} u(x_{ij}) < 0$$

since $\forall i\in I$, $i\in B$. But, again, this yields a contradiction, implying that $A$ is non-empty. Thus sufficiency is established.

To establish necessity, we demonstrate that: (1) if $B$ is empty, vote trading is not profitable and (2) if $A$ is empty, sincere voting would not improve upon the results of vote trading. Assume that $B$ is empty. Then

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) \leq 0.$$

But this implies that

$$\sum_{i}\sum_{j} u(\bar{x}_{ij}) \leq 0$$

since $\forall i\in I$, $i\in A$. However, the condition for vote trading is

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) > 0$$

and we have

$$\sum_{i}\sum_{j} u(\bar{x}_{ij}) > 0.$$

But, by assumption

$$\sum_{i\in A}\sum_{j} u(\bar{x}_{ij}) + \sum_{i\in B}\sum_{j} u(x_{ij}) \leq 0.$$

Thus, if $B$ is empty, the condition for vote trading must be violated. Hence, $B$ is non-empty. Now assume that $A$ is empty. Then,

$$\sum_{i\in B}\sum_{j} u(x_{ij}) \geq \sum_{i}\sum_{j} u(\bar{x}_{ij})$$
$$\rightarrow \sum_{i}\sum_{j} u(x_{ij}) \geq \sum_{i}\sum_{j} u(\bar{x}_{ij})$$

since $\forall i\in I$, $i\in B$. If the strict inequality holds, we have:

$$\sum_{i}\sum_{j} u(x_{ij}) > \sum_{i}\sum_{j} u(\bar{x}_{ij})$$

which implies that sincere voting would improve upon the results of vote trading. However,

$$\sum_{i}\sum_{j} (\bar{x}_{ij}) = 0$$

by assumption and thus

$$\sum_{i}\sum_{j} u(x_{ij}) > 0.$$

This contradicts our assumption that

$$\sum_{i\in B}\sum_j u(x_{ij}) \le 0,$$

since $\forall i \in I, i \in B$. Thus.

$$\sum_i\sum_j u(x_{ij}) = \sum_i\sum_j u(\tilde{x}_{ij}),$$

establishing necessity.

Finally we have:

Let

$$\sum_i\sum_j u(\tilde{x}_{ij}) = 0 \text{ and } \sum_i\sum_j u(x_{ij}) < 0.$$

For trades to occur, we have:

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij}).$$

For nontraders. by stipulation:

$$\sum_{i\in B}\sum_j u(x_{ij}) < 0.$$

To establish that vote trading is profitable under these conditions—the proof of sufficiency—we must show: (1) $A$ is non-empty and (2) $B$ may be empty. We need not demonstrate that $B$ is always empty since that would imply that the *only* profitable trading situation occurs when all players trade, which is not maintained here.

Thus, the conditions for trading and nontrading give:

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) + \sum_{i\in B}\sum_i u(x_{ij}) \le 0.$$

Let

$$\sum_i\sum_j u(\tilde{x}_{ij}) = 0 \quad \text{and} \quad \sum_i\sum_j u(x_{ij}) < 0.$$

Assume first that $A$ is empty. Then

$$\sum_{i\in B}\sum_j u(x_{ij}) = \sum_i\sum_j u(x_{ij}) < 0$$

since $\forall i \in I, i \in B$. If $A$ is empty, however, the condition for vote trading

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij})$$

must be violated. The violation of the condition for vote trading, however, contradicts our initial premise that

$$\sum_i\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij}).$$

Thus, $A$ is non-empty. Now suppose that $B$ is empty. Since $A$ is non-empty,

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) \le 0$$

and

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij}) \rightarrow \sum_i\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij})$$

if $B$ is empty. Since this result does not contradict our initial premise, $B$ may be empty and sufficiency is established.

To prove necessity we must show that

$$\sum_i\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij})$$

if: (1) $A$ is non-empty and (2) $B$ may be empty. If $B$ is empty, then

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) = \sum_i\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij})$$

by the condition for vote trading. Assume that $A$ is empty. Then, the condition for vote trading must be violated and:

$$\sum_{i\in A}\sum_j u(\tilde{x}_{ij}) \le \sum_i\sum_j u(x_{ij}) \rightarrow \sum_{i\in B}\sum_j u(x_{ij}) = \sum_i\sum_j u(x_{ij}) \ge \sum_i\sum_j u(\tilde{x}_{ij}).$$

Thus,

$$\sum_i\sum_j u(\tilde{x}_{ij}) > \sum_i\sum_j u(x_{ij})$$

only if $A$ is non-empty. Necessity is established.

## Appendix II

The distribution of tastes which Riker and Brams discuss (see note 24 above) is one which yields a cyclical majority. It can readily be confirmed that a cycle is found in Table 1 above. Particularly in the case of cyclical majorities, the strategy of implicit vote trading (always trading with the same partner) is not likely to yield optimal results. Implicit vote trading on a large scale implies a cohesive voting system (and the potential for responsible parties). Such a system is realizable only if the distribution is single-peaked. Riker and Brams ("The Paradox of Vote Trading," pp. 1244–1246) discuss this problem. A specific set of illustrations will help. Let $u_i(1, 2)$ denote the utility to voter $i$ obtained from an implicit vote trading agreement between members 1 and 2, etc. Then, for simple majority rule, we have:

$$\begin{array}{lll}
u_1(1, 2) = 3 & u_1(1, 3) = 3 & u_1(2, 3) = -10 \\
u_2(1, 2) = 3 & u_2(1, 3) = -10 & u_2(2, 3) = 3 \\
u_3(1, 2) = -10 & u_3(1, 3) = 3 & u_3(2, 3) = 3
\end{array}$$

For a unanimity voting rule, we have:

| | | |
|---|---|---|
| $u_1(1, 2) = -1$ | $u_1(1, 3) = -1$ | $u_1(2, 3) = 8$ |
| $u_2(1, 2) = -1$ | $u_2(1, 3) = 8$ | $u_2(2, 3) = -1$ |
| $u_3(1, 2) = 8$ | $u_3(1, 3) = -1$ | $u_3(2, 3) = -1$ |

In the case of majority rule, implicit trading is profitable for both trading partners, but no single coalition dominates any of the others. Since the expected value of the three coalitions is equal to $-4$, the three coalitions are not even preferable to the results obtained from the paradox of vote trading. None of these coalitions, however, is dominated by the results from simple majority rule without vote trading, so that all three possible coalitions remain in the core of the voting game. For a unanimity rule, note that the nontrader still benefits from the trading activity of the others. The $+8$ entries for the nontrader in each coalition are simply the values of his security level (see Tables 5 and 6 above). In each case, the traders are worse off than if no logrolling had occurred—but actually are better off than if they had attempted to employ "explicit" sophisticated voting strategies. But, the outcomes for sophisticated voting are themselves not in the core for the voting game under a unanimity rule, whereas a unanimity rule with vote trading dominates each of the three solutions for "implicit" vote trading. Thus, the coalitions from implicit vote trading are not in the core for a unanimity voting rule—and neither is the "actual" solution of the voting game: a refusal to trade by each player. This "solution" is in turn dominated by each incidence of implicit vote trading. The paradoxical result is that the only solution considered which is in the core is not attainable. Hence, we have the paradox of vote trading and the contradiction is even more clearly defined for a unanimity rule than it is for a majority decision rule (in which implicit vote trading solutions are indeed in the core). This is the case because, as we argue above (see note 19), the vote trading situation under a unanimity rule is a better illustration of the traditional Prisoners' Dilemma than is the situation under majority rule.

# The Instability of Minimum Winning Coalitions*

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"In *n*-person, zero-sum games, where side payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur."[1]

This is William Riker's statement of the size principle as it first appeared in his ground-breaking book, *The Theory of Political Coalitions.* It has been perhaps the most widely accepted and recognized finding of the new school of deductive political science. Yet recently, other political scientists have begun to express doubts about its validity and to adduce arguments calling it into question.[2]

Robert Butterworth has demonstrated that rational individuals might form larger than minimum winning coalitions in symmetric zero-sum games. Using a five-person game as a counter-example he has demonstrated that if a three-person minimum coalition is about to form it can be to the advantage of one of the potential losers to bribe his way into the coalition. He also shows that it is to the advantage of the coalition members to allow themselves to be bribed and that the one remaining individual who expects to be left outside the expanded coalition cannot rationally expect to entice the briber back into the losing coalition by offering to bribe the briber. His conclusion is, therefore, that larger than minimum winning coalitions are possible.

In a recent article and series of exchanges with Butterworth, Kenneth Shepsle has challenged some aspects of this argument.[3] He points out that Butterworth failed to take into account the possibility that the one individual who is in danger of being left out of the coalition also has the option of entering into competitive bribery with the initial briber for the favor of being allowed into the minimum winning coalition. Shepsle concludes that such competitive bribery would place the two potential losers in such an unfavorable position that they would find it advantageous to recoalesce together thereby allowing the minimum coalition of three to form. He acknowledges, however, that another round of competitive bribery could start at any time and that the minimum coalition configuration is unstable.

The purpose of this paper is to generalize and extend the findings of the authors discussed above beyond five-person games to *n*-person games. It will be shown that for a general class of essential, symmetric zero-sum games with side payments, larger than minimum winning coalitions are compatible with rational behavior on the part of individuals.[4] Moreover, it will be demonstrated that only in a highly restricted class of games will minimum winning coalitions be immune to the bribery of the form identified by Butterworth.

To prove these assertions I will examine an essential symmetric zero-sum game and specify the conditions under which some subset of players not in the minimum winning coalition have an incentive to make an offer to the winners to be let into the coalition which the winners will find profitable to accept. Under these conditions it will be demonstrated that the residuum of losers (those not attempting to get in) will have an incentive to make a counteroffer to the potential defectors to keep them out of the winning coalition. Moreover it will be shown that the best possible return to the potential defectors will be the same for the two alternatives available, (i.e. join-

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* I have been the beneficiary of a great deal of helpful inspiration and criticism in the preparation of this paper. The impetus for it was provided by an idea in a preliminary draft of Oran Young's paper, "The Size of Winning Coalitions: A Note on Riker's Size Principle." The argument has been refined and expanded as a result of truly helpful comments from Lawrence Dodd, Russell Hardin, Joe Oppenheimer, Kenneth Shepsle, Jeff Smith, and Harrison Wagner. I am deeply grateful for their help. An earlier version of this paper was presented at the annual meeting of the Public Choice Society, New Haven, Connecticut, March 1974. Research on this paper was supported by National Science Foundation Grant #GS-33490.

[1] William Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), p. 32.

[2] Robert Lyle Butterworth, "A Research Note on the Size of Winning Coalitions," *American Political Science Review*, 65 (September, 1971), 741–745, and Russell Hardin, "Hollow Victory: The Minimum Winning Coalition," Fels Center of Government, Discussion Paper #30. Butterworth calls Riker's findings into question via a theoretical argument, whereas Hardin presents a different theoretical argument as well as a discussion of the empirical problems inherent in any attempt to test Riker's thesis. Although both make telling points the argument here concentrates on extending Butterworth's line of reasoning.

[3] Kenneth Shepsle, "On the Size of Winning Coalitions," *American Political Science Review*, 68 (June, 1974), 505–518, Robert Lyle Butterworth, "Comment on Shepsle's 'On the Size of Winning Coalitions,' " and Shepsle, "Minimum Winning Coalitions Reconsidered: A Rejoinder to Butterworth's 'Comment'," *Ibid.*, 519–524.

[4] Riker's argument was articulated in terms of symmetric games, as can be seen from his use of a graphical representation of the characteristic function. This paper discusses only symmetric games of the type treated by Riker.

ing the minimum winning coalition or being paid to stay out of the minimum winning coalition). Thus if bidding proceeds in this fashion, larger than minimum winning coalitions are possible, but the situation is indeterminate.

It will be further demonstrated that even if the residuum of losers try competitively to bid their way into the winning coalition (rather than attempting to bribe the defectors into remaining in the losing coalition) the situation is no more determinate. A cyclical bribery situation would result which could arbitrarily terminate at any size leaving larger than minimum winning coalitions.

Finally it will be shown that the conditions which allow for the onset of a potentially cyclic and indeterminate round of bribery are general. But the conditions necessary to preclude the possibility of such bribery, and so to preclude the formation of larger than minimum winning coalitions, will be shown to be quite restrictive. Only in games in which the potential set of losers has no incentive to form any countercoalitions is this form of bribery ruled out. This special class of games will be called "games inessential over coalitions of losers." Only in these games can Riker's principle be supported. The conclusion therefore is that in most symmetric zero-sum games larger than minimum winning coalitions can form.

To see how this result follows consider the following game. Let $v(i)$ represent the value of a coalition of $i$ members. Then to specify the game as an essential $n$-person, zero-sum symmetric game we require that:

(1) $v(i)$ be defined over the integers $0 \leq i \leq n$ $(n \geq 5)$;[5]
(2) $v(i) = -v(n-i)$;
(3) for all positive integers $i$ and $j$ such that $i+j \leq n$, $v(i)+v(j) \leq v(i+j)$;
(4) for some $i$ and $j$, $v(i)+v(j) < v(i+j)$.

Condition 1 is simply the requirement that the payoffs to each size coalition be specified, i.e. that the game is well defined and symmetric. Condition 2 is the zero-sum condition. Condition 3, superadditivity, is the requirement that any coalition can do at least as well as the sum of its parts; no loss is incurred to the members of two coalitions if they coalesce. Condition 4 is simply the requirement that there is at least one coalition which actually makes the members better off than they could be if they failed to coalesce but remained in smaller coalitions. In the absence of Condition 4 there is never any incentive for any coalitions to form.

To develop the argument, let $S$ represent the minimum winning coalition of $s$ members where $s \leq n-2$. There are therefore, at least two potential losers. Let $-S$ be the losing coalition. Then let $r$ be any integer such that $1 \leq r < n-s$ and call some set of $r$ individuals in $-S$, $R$. Thus $R$ is a subset of losers but not all of the losers. Call the remaining subset of losers in $-S$, $Q$. For convenience let us refer to the number of individuals in $Q$ as $q$, bearing in mind that $q+r=n-s$. Then we have a minimum winning coalition $S$ and a partition of the losers into two proto-coalitions $R$ and $Q$.

Assume that the minimum winning coalition $S$ has tentatively formed, and in keeping with the symmetry of the game has agreed on a symmetric distribution of the rewards of $v(s)$. Suppose that the members of $R$ were to attempt to offer the members of $S$ some bribe to be split among them in a symmetric fashion. What would be the necessary conditions for the members of $R$ to be able to make an acceptable bid to the members of $S$ to be let into the winning coalition? Such a condition is the existence of an "a"—i.e., the amount that the members of $R$ would give to the members of $S$, (or get from them if $a$ were negative), which allows the following inequality to hold:[6]

$$\begin{aligned} S\text{'s new payoff} &= v(s+r) + a > v(s) \\ &= S\text{'s old payoff or} \qquad (1) \\ v(s+r) - v(s) &> -a. \end{aligned}$$

But, of course another necessary condition for members of $R$ to make such a bid is the requirement that they would be better off if it is accepted than they would be if the losing coalition symmetrically shared the cost of the losers.[7] This condition is:

$$\begin{aligned} R\text{'s new payoff} &= -a > -\frac{r}{r+q}\,v(s) \qquad (2) \\ &= R\text{'s old payoff.} \end{aligned}$$

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[5] In this paper I examine only games involving five or more players. This allows us to examine competitive bribery when there are at least two potential losers. All of the results not involving competitive bribery can be easily demonstrated for games with fewer than five members.

[6] Here for the sake of convenience I treat the division of payoffs between $S$ and $R$ as if $S$ appropriated to itself all of the winnings of $v(s+r)$ and in addition got a bribe of amount "$a$" from $R$ for the formation of $S \cup R$. The use of the term "bribe," however, may be misleading. If, for example, the game in question were of positive slope over the range $s$ to $s+r$ then the members of $S$ might actually offer the members of $R$ a "bribe" to secure the formation of $S \cup R$. In that event "a" would be a negative quantity. In either event other ways of representing the division of payoffs are possible. One could posit that the members of $S \cup R$ divided the payoffs symmetrically and that in addition the members of $R$ gave the members of $S$ a bribe of size "$b$". The analysis would proceed in the same way as it does in this representation. Only the algebra would be different. The conclusions would be the same.

[7] The assumption that the "losers" initially expect to share the losses symmetrically is not necessary for the argument which follows. It is a convenience, adopted for ease of exposition. All the conclusions that follow can be derived without that assumption.

Conditions (1) and (2) can be combined to yield condition (3) which is necessary for a mutually acceptable bid:

$$v(s+r) - v(s) > -a > -\frac{r}{r+q}v(s) \quad \text{or}$$
$$v(s+r) > \frac{q}{r+q}v(s). \tag{3}$$

Notice that (3) is simply a restriction on the slope of the characteristic function over the range $s$ to $s+r$. Condition (3) always holds if the slope over that range is positive and it specifies a maximum rate of drop off which allows $R$ to bid successfully.

By a similar argument we could derive the condition on the characteristic function that would allow $Q$ to make a mutually acceptable bid to $S$ to be let into the minimum winning coalition. That condition is:

$$v(s+q) > \frac{r}{r+q}v(s) \tag{3'}$$

Now suppose that condition (3) held and hence that $R$ could offer $S$ a bribe. Could this result in a larger than minimum winning coalition? If $R$ offered $S$ a bribe one possibility is that $Q$ might offer $R$ a bribe *not* to enter into the coalition with $S$. The coalition $Q$ could offer $R$ any amount $a'$ consistent with the inequality:

$$\begin{aligned} &Q\text{'s position if } R \text{ accepts } Q\text{'s bribe}\\ &\quad = -a' + v(r+q) > v(q)\\ &\quad = Q\text{'s position if } R \text{ joins } S\\ \text{or} &\quad -a' > v(q) - v(r+q). \end{aligned} \tag{4}$$

Using the zero-sum condition on (4) it becomes:

$$a' < v(s+r) - v(s). \tag{5}$$

Thus $a'$ is $R$'s net payoff position as offered by $Q$. From (5) it follows that if $Q$ is interested in getting $R$ to stay out of a coalition with $S$ they can offer $R$ a net payoff position which approaches $v(s+r) - v(s)$. The best return $R$ could get from $Q$ would approach that amount. But from (1) the net return that $R$ could get from entering into a coalition with $S$, $-a$, also has an upper limit of $v(s+r)-v(s)$. Thus, if $S$ and $Q$ compete for $R$ as coalition partners the best bids that each could make to $R$ would have the same upper limit of $v(s+r)-v(s)$. One cannot say whether $R$ will join with $S$ or remain in $-S$. Under these bidding conditions one cannot say whether the winning coalition which will form is the minimal $S$ or $S \cup R$.

But as Shepsle has pointed out, the options open to the set $Q$ are not necessarily restricted to attempting bribery of the members of $R$ to keep them from coalescing with $S$. The members of $Q$ also have open the option of attempting to competitively outbid $R$ for the privilege of coalescing with $S$. Suppose that $Q$ were to attempt such a counterbid? Since $Q$ faces the possibility of receiving $-v(s+r)$ if $R$ succeeds in its bid, $Q$ would conceivably be willing to offer up to $v(s+r)$ to be let into the coalition with $S$. Were $S$ to accept such a bid and form $S \cup Q$ the net return to $S$ would have an upper bound of:

$$v(s+q) + v(s+r).$$

But such an outcome would leave the members of $R$ in the lurch. Their net return under such an arrangement would be $-v(s+q)$. To forestall such an eventuality they might be tempted to up the amount they would be willing to offer $S$. They conceivably would be willing to offer up to $v(s+q)$ for the privilege of forming $S \cup R$. Under their maximum rational offer the net return to $S$ would approach:

$$v(s+r) + v(s+q).$$

Now each of these maximal bids to $S$ are of the same size. There is no way of concluding that one or another of the two coalitions $S \cup R$ or $S \cup Q$ will be more attractive to $S$ and will, therefore, form. Moreover, following Shepsle's argument, one can notice that $Q$ and $R$, by bidding for $S$'s favor on the basis of the worst that might befall them if the other were to join with $S$, may well put themselves in a position that could be improved to their mutual benefit were they to fall back into the coalition $R \cup Q$. To see this, notice that their joint return when they outbid one another amounts to $-v(s+q)-v(s+r)$ or, by the zero-sum condition, $v(r)+v(q)$, which is the worst they possibly could do acting on their own as coalitions. If the game in question is essential, to the extent that $v(r)+v(q)<v(r+q)$, then they would have an incentive to form $R \cup Q$ to cut their losses and to get $v(r+q)$. In such a case when $S$ is a minimum winning coalition one might find the tendency identified by Shepsle to move toward a minimum winning coalition again, but it would not necessarily follow. For $S$ would have an incentive to prevent the defection of the winner of the competitive bidding. $S$ would conceivably enter into a bidding contest to stop the possible regrouping of $R$ and $Q$. This situation would then resemble one of the bidding situations identified above, and the size of the resultant coalition would not necessarily be the minimum winning size.

But of course this argument was predicated on the assumption that condition (3) held for some $r$ and $q$. If the condition holds then larger than minimum winning coalitions are possible. Similarly, if condition (3′) holds, larger than minimum coalitions could form due to $Q$'s initial bidding. Only if neither (3) nor (3′) hold for any $r$ and $q$ is such bidding precluded and one can expect only minimum winning coalitions to form.

Let us then examine the characteristics of games for which neither inequality (3) or (3′) hold for any $r$ and $q$. If (3) does not hold then for any $r$ and $q$

$$v(s + r) \leq \frac{q}{r + q} v(s), \qquad (6)$$

and if (3′) does not hold, then

$$v(s + q) \leq \frac{r}{r + q} v(s). \qquad (6')$$

But it can be shown that the strict portions of the inequalities in (6) and (6′) cannot hold. To see this notice that from (6) by the application of the super-additivity condition one can derive:

$$v(s) + v(r) \leq v(s + r) \leq \frac{q}{r + q} v(s),$$

which implies

$$\frac{r}{r + q} v(s) \leq - v(r),$$

and by applying the zero-sum condition we get

$$\frac{r}{r + q} v(s) \leq v(s + q). \qquad (7)$$

Now comparing (6′) and (7), it is apparent that strict equality must hold in both for them to be consistent. Thus the negation of conditions (3) and (3′) imply that:

$$v(s + q) = \frac{r}{r + q} v(s). \qquad (8)$$

Similarly it can be shown that:

$$v(s + r) = \frac{q}{r + q} v(s). \qquad (8')$$

Adding (8) and (8′) we obtain the restriction on the characteristic function necessary to preclude the cyclic bribery. It is

$$v(s + q) + v(s + r) = v(s).$$

Applying the zero-sum condition to this equation yields the condition:

$$v(r) + v(q) = v(r + q). \qquad (9)$$

Equation (9) must hold for all partitions of losers, i.e., all partitions of $-S$, if only minimum winning coalitions are to be expected. If we call games for which (9) holds for all $r$ and $q$ such that $r+q=n-s$ "inessential over coalitions of losers," we can restate our result succinctly. Only in games which are inessential over coalitions of losers can minimum winning coalitions be expected to form exclusively.

Once identified, this result is quite intuitive. If a game does not conform to (9) then the losers have an incentive to form a countercoalition. The incentive consists of a potential cut in their losses. But the saving to be gained by forming a countercoalition would be accomplished at the expense of the minimum winning coalition. Consequently, members of the minimum winning coalition have an incentive to stop the formation of this countercoalition, or to break it if it has formed. They could do so by offering to bring some of the losers into their coalition and by sharing with them some of the losses thereby saved. The losers left out in the cold by such a maneuver could attempt to lure back the defectors or to outbid them for the favors of the minimum winning coalition. In either case the size of the resulting coalition is indeterminate and potentially larger than minimum winning size. Only if there are no potential savings to losers from forming a countercoalition is there no incentive for the bribery to begin.

This analysis has implications for future empirical research into the formation of minimum winning coalitions. Although minimum winning coalitions cannot be expected to form in general, there is a theoretically restricted class of cases in which we would expect to find only such coalitions. Moreover, this set may still be empirically interesting. For example, the formation of multiparty cabinets in some legislatures may be situations in which those excluded stand to gain nothing by coalescing among themselves. To the extent these are zero-sum games one should expect only minimum winning coalitions to form in these cases. On the other hand if, in some other legislatures, losers stand to gain from coalescing, then larger than minimum winning cabinet coalitions can be expected.[8] The payoff structure for losers, therefore, is an important variable to be considered in any prediction of coalition behavior and any future testing of hypotheses dealing with the formation of minimum winning coalitions.

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[8] Differences in the payoff structures in legislatures in different countries might explain some of the variance in cabinet size found by researchers such as Lawrence Dodd, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis," *American Political Science Review*, 68 (September, 1974), 1093–1117.

# Evaluation of a Presidential Election Game

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## 1. Introduction

There has been considerable discussion, over the past decade, of the merits of the presidential election system in the United States. In particular, discussion has frequently centered on the excessive power which this system seems to give one group or another (the large states, the small states, organized minorities within one or another of these kinds of states, etc.), though there is also frequent disagreement about the identity of these favored groups.

In this paper we carry out an evaluation of the presidential election game. The Shapley value[1] is computed approximately by the method of multilinear extensions.[2] A comparison with the Banzhaf value[3] will also be made.

The following simplifying assumptions will be made:

(a) The number of voters within a state is directly proportional to that state's population, according to the last census.

(b) The presidential electors for each state are pledged to the candidate who carries that state, and have no choice but to abide by their pledge.

(c) The election will be decided by the electors rather than reaching the House of Representatives.

The reason for assumption (a) is that it allows us to work directly with census figures rather than having to count voter rolls, and having to figure out the number of potential but unregistered voters. The assumption is reasonable, in the sense that we might expect voters to be approximately proportional to total population. There may, however, be certain states with large alien populations for which this assumption is not very accurate. Additionally, we will find that toward the end of the decade, a fast-growing state (e.g., Florida or California) is underrepresented in the Electoral College (i.e., by the 1980 election, Florida's population may be much larger than represented by the apportionment which was based on 1970 census figures).

Assumption (b) also seems to be reasonable. Admittedly the electors are (constitutionally) free to vote for anyone, and in the last few elections some have actually done so. Nevertheless it seems doubtful that this independence would be exercised in a truly close election.

Assumption (c), of course, does not always hold. Nevertheless, referral to the House of Representatives occurs so infrequently (this last happened after the 1824 election) that we consider its probability negligible. (It should be noted that in 1968, when this possibility seemed to be somewhat probable, Mr. Wallace seemed more interested in making a deal *within* the Electoral College than in letting the election be decided in the House.)

## 2. Game-Theoretic Basis

A game, as understood here, is merely a pair $(N; v)$ where $N$ is a (finite) set of players, and $v$ is a function which assigns to each subset $S \subset N$, a number $v(S)$. The subsets are called coalitions; the elements $i$ of N are players.

The presidential election can be thought of as a game $(N; u)$, where $N$ is the set of all voters in the country, and $u$ assigns, to each coalition $S$ of voters, the number 0 or 1, depending on whether $S$ is "large" enough to win the election against its complement, $N-S$. Thus, $u(S)=1$ if $S$ contains a majority of the voters within each of a set $T$ of states, these states (in $T$) having at least 270 electoral votes. Otherwise, $u(S)=0$.

For an arbitrary game $(N; v)$, the Shapley value is a vector $\mathcal{G}[v]$, whose components $\mathcal{G}_i[v]$ are given by

$$\mathcal{G}_i[v] = \sum_{\substack{S \subset N \\ i \notin S}} \frac{s!(n-s-1)!}{n!} [v(S \cup \{i\}) - v(S)] \tag{2.1}$$

where $s$ and $n$ are the number of players in $S$ and $N$, respectively. This component $\mathcal{G}_i[v]$ is known as the *value to player i of the game v*, and represents (in some sense) his power within that game. It is this value which we shall study for the presidential election game.

As can be seen, the formula (2.1) requires the sum of a very large number of terms (i.e., one for each coalition which does not contain $i$) and is therefore quite difficult to compute when $N$ is large. In particular, for a presidential election, where $N$ is of the order of $10^8$, a direct evaluation seems out of the question. Recourse is therefore frequently had to the multilinear extension (MLE) of the game. Essentially, the MLE of game $(N; v)$ is the function $f(x_1, \cdots, x_n)$ defined by

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[1] Lloyd S. Shapley, "A Value for n-Person Games," in *Annals of Mathematics Studies No. 28* (Princeton: *Princeton University Press* 1953), 307–327.

[2] Guillermo Owen, "Multilinear Extensions of Games," *Management Science*, 18 (1972), 64–79.

[3] John F. Banzhaf III, "One Man, 3.312 Votes: A Mathematical Analysis of the Electoral College," *Villanova Law Review*, 13 (1968), 304–332.

$$f(x_1, \cdots, x_n) = \sum_{S \subset N} \prod_{i \in S} x_i \prod_{i \notin S} (1 - x_i) v(S). \tag{2.2}$$

It is shown in an earlier paper by the author[4] that, if $f$ is the MLE of $v$, then

$$\mathcal{g}_i[v] = \int_0^1 f_i(t, t, \cdots, t)\, dt \tag{2.3}$$

where $f_i$ is the $i$-th partial derivative $\partial f / \partial x_i$.

The advantage of using the MLE is that it can be given a probabilistic interpretation. Thus, $f(x_1, \cdots, x_n)$ is the mathematical expectation

$$E[v(\bar{\bar{S}})],$$

where $S$ is a random coalition, determined probabilistically by saying that there is probability $x_1$ that player $i$ will belong to $\bar{\bar{S}}$, and that the probabilities of different players belonging are independent. This interpretation will allow us to give approximations based on the central limit theorem.

### 3. The State Game

If we disregard the process by which the electors are themselves chosen, we can think of the electoral college as itself representing a game. By assumption 1(b) above, all the electors from a given state will vote for the same candidate, and so each state can be thought of as a single player. We have, then, here, a *weighted majority game* $(M; v)$, where $M$ is the set of all states (plus the District of Columbia), and, for any $T \subset M$,

$$v(T) = \begin{cases} 0 & \text{if } \sum_T w_j \leq 269 \\ 1 & \text{if } \sum_T w_j \geq 270 \end{cases} \tag{3.1}$$

$w_j$ being the number of electoral votes that state $j$ has.

For this 51-person game, a direct computation of the Shapley value (2.1) still seems out of the question. Use can, however, be made of certain properties of the game (e.g., that $v$ has only the values 0 or 1, and that $v(T)$ depends only on $\sum_T w_j$) to obtain an exact result through a method of generating functions. The exact details are given in Shapley and Mann[5] as calculated for the Electoral College after the 1950 and 1960 census reapportionments.

As against this exact method, we may approximate the Shapley value by the method of MLE's. In fact, the partial derivative $f_i(x_1, \cdots, x_n)$ can be expressed as

$$f_i(x_1, \cdots, x_n) = E[v(\bar{\bar{S}} \cup \{i\}) - v(\bar{\bar{S}})], \tag{3.2}$$

where $\bar{\bar{S}}$ is a random coalition, such that player $j (j \neq i)$ has probability $x_j$ of belonging to $\bar{\bar{S}}$, and that these probabilities are independent. (Here, $\bar{\bar{S}}$ is assumed not to contain $i$.) Now, in 3.2, each of the terms $v(\bar{\bar{S}} \cup \{i\})$ and $v(\bar{\bar{S}})$ equals either 0 or 1, and $v(\bar{\bar{S}}) \leq v(\bar{\bar{S}} \cup \{i\})$, so that the variable $v(\bar{\bar{S}} \cup \{i\}) - v(\bar{\bar{S}})$ can have only the two values 0 or 1. Its expectation, then, is the probability that it equal 1, and this happens only if $v(\bar{\bar{S}}) = 0$ but $v(\bar{\bar{S}} \cup \{i\}) = 1$. By (3.1), this happens if

$$\sum_{\bar{\bar{S}}} w_j \leq 269$$

and

$$w_i + \sum_{\bar{\bar{S}}} w_j \geq 270.$$

Thus,

$$f_i(x_1, \cdots, x_n) = \text{Prob}\left\{270 - w_i \leq \sum_{\bar{\bar{S}}} w_j \leq 269\right\} \tag{3.3}$$

Let, now, $Y = \sum_{\bar{\bar{S}}} w_j$. Then $Y$ is a random variable, equal to the sum of 50 variables $Z_j$, where

$$Z_j = \begin{cases} 0 & \text{if } j \notin \bar{\bar{S}} \\ w_j & \text{if } j \in \bar{\bar{S}}. \end{cases}$$

As we have seen above, the variables $Z_j$ are independent. $Z_j$ has the values 0 and $w_j$ with probabilities $1 - x_j$ and $x_j$, respectively. Thus $Z_j$ has mean and variance

$$\mu(Z_j) = x_j w_j \qquad \sigma^2(Z_j) = x_j(1 - x_j) w_j^2 \tag{3.4}$$

and $Y$, being the sum of these, has mean and variance

$$\mu(Y) = \sum_{j \neq i} x_j w_j \tag{3.5}$$

$$\sigma^2(Y) = \sum_{j \neq i} x_j (1 - x_j) w_j^2. \tag{3.6}$$

Since $Y$ is the sum of many (50) independent random variables, all with similar distributions, it is possible to approximate $Y$ by a normal random variable having the mean (3.5) and variance (3.6). Since $Y$ has only integer values, but the normal variable is continuous, a correction is made, setting 269.5 (rather than 270) as the necessary number of votes. Then we have the approximation

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[4] Owen, "Multilinear Extensions."

[5] Lloyd S. Shapley and Irwin Mann, "Values of Large Games, VI: Evaluating the Electoral College Exactly," RM-3158-PR, The RAND Corporation, May 1962.

$$3.7 \qquad f_i(x_1, \cdots, x_n) \cong \text{Prob}\left\{269.5 - w_i \leq Y' \leq 269.5\right\},$$

where $Y'$ is a normal variable with the mean (3.5) and variance (3.6).

Applying formula (2.3), now we notice that, if $x_1 = x_2 = \cdots x_n = t$, then (3.5) and (3.6) take the form

$$3.8 \qquad \mu(Y) = t \sum_{j \neq i} w_j$$

and

$$3.9 \qquad \sigma^2(Y) = t(1-t) \sum_{j \neq i} w_j^2.$$

The righthand side of (3.7) can now be evaluated (for given values of $t$) by means of a table of the normal distribution; it is then necessary to integrate this (numerically), to obtain the power of state $i$.

In Table 1, values are given both by the exact method and by the MLE approximation, for the 1970 apportionment. The approximation, while not exact, is really not bad. (Maximum error 0.367 per cent.)

## 4. The Composed Game

We proceed now to the presidential election game. This can best be described by saying that it is the composition $v[u_1, u_2, \cdots, u_{51}]$ of the game $v$, described in section 3, with the 51 games $u_j$ which are played within the individual states, i.e., a game is played within each state to determine the state's electors, who then vote for the presidential candidate according to the game $v$.

Each of the games $u_j$ is very easy to handle—in fact, $u_j$ is a simple majority game for $m_j$ players—$m_j$ being the number of voters in the state, which we assume proportional to the state's population. Thus

$$u_j(S) = \begin{cases} 0 & \text{if } s \leq \dfrac{m_j}{2} \\ 1 & \text{if } s > \dfrac{m_j}{2} \end{cases}$$

where $s$ is the number of players in $S$.

It is, of course trivial—by symmetry—to compute the Shapley value for each of the games $u_j$. Unfortunately, composition of games does not generally give rise to a composition of values, and so it is not possible to obtain exact values for the composed game.

Turning to the MLE's, we find, however, that MLE's do compose when the games compose. Thus, if the game $v$ has MLE $f$, while the games $u_1, \cdots, u_{51}$ have MLE's $g_1, \cdots, g_{51}$ respectively, then the compound game $v[u_1, \cdots, u_{51}]$ has the MLE $h$, where

**Table 1**

| Electoral Votes | Number of States | Exact Value $\mathcal{J}_i$ | Rescaled Index 538 $\mathcal{J}_i$ | MLE Approximation $\mathcal{J}_i'$ | Approximation Ratio $\mathcal{J}_i'/\mathcal{J}_i$ |
|---|---|---|---|---|---|
| 45 | 1 | .08830938 | 47.510446 | .08852076 | 1.00239 |
| 41 | 1 | .07972734 | 42.893309 | .07975829 | 1.00039 |
| 27 | 1 | .05096311 | 27.418153 | .05113219 | 1.00332 |
| 26 | 2 | .04897682 | 26.349529 | .04915304 | 1.00360 |
| 25 | 1 | .04699893 | 25.285424 | .04715678 | 1.00336 |
| 21 | 1 | .03916926 | 21.073062 | .03927665 | 1.00274 |
| 17 | 2 | .03146563 | 16.928509 | .03157240 | 1.00339 |
| 14 | 1 | .02576694 | 13.862614 | .02586151 | 1.00367 |
| 13 | 2 | .02388196 | 12.848494 | .02396389 | 1.00343 |
| 12 | 3 | .02200413 | 11.838222 | .02207416 | 1.00318 |
| 11 | 1 | .02013337 | 10.831753 | .02019398 | 1.00301 |
| 10 | 4 | .01826959 | 9.829039 | .01833326 | 1.00349 |
| 9 | 4 | .01641273 | 8.830049 | .01647300 | 1.00367 |
| 8 | 2 | .01456270 | 7.834733 | .01461150 | 1.00335 |
| 7 | 4 | .01271944 | 6.843059 | .01276382 | 1.00349 |
| 6 | 4 | .01088284 | 5.854968 | .01092260 | 1.00365 |
| 5 | 1 | .00905301 | 4.870519 | .00907755 | 1.00271 |
| 4 | 9 | .00722957 | 3.889509 | .00724257 | 1.00180 |
| 3 | 7 | .00541245 | 2.911898 | .00543059 | 1.00335 |
| Total | 51 | 0.99999997 | 537.999803 | 1.00296983 | |

Table 2

| | Population $m_i$ | Electoral Votes $w_i$ | Total Value $\Psi_i$ | Value Per Inh. $\Psi_i/m_i$ ($\times 10^{-9}$) | Ratio $v_i/v_{\text{d.c.}}$ | Banzhaf Ratio $\beta_i$ |
|---|---|---|---|---|---|---|
| 1960 | | | | | | |
| Alabama | 3,266,740 | 10 | .013971 | 4.2767 | 1.615 | 1.632 |
| Alaska | 226,167 | 3 | .0011068 | 4.8937 | 1.845 | 1.838 |
| Arizona | 1,302,161 | 5 | .0044025 | 3.3809 | 1.275 | 1.281 |
| Arkansas | 1,786,272 | 6 | .0062101 | 3.4766 | 1.313 | 1.315 |
| California | 15,717,204 | 40 | .130848 | 8.3251 | 3.143 | 3.162 |
| Colorado | 1,753,947 | 6 | .0061309 | 3.4955 | 1.321 | 1.327 |
| Connecticut | 2,535,234 | 8 | .0098302 | 3.8774 | 1.464 | 1.477 |
| Delaware | 446,292 | 3 | .0015535 | 3.4809 | 1.313 | 1.308 |
| Dist. of Columbia | 763,956 | 3 | .0020251 | 2.6508 | 1.000 | 1.000 |
| Florida | 4,951,560 | 14 | .024200 | 4.8873 | 1.845 | 1.870 |
| Georgia | 3,943,116 | 12 | .018404 | 4.6674 | 1.762 | 1.789 |
| Hawaii | 632,772 | 4 | .0024692 | 3.9022 | 1.472 | 1.468 |
| Idaho | 667,191 | 4 | .0025366 | 3.8019 | 1.434 | 1.429 |
| Illinois | 10,081,158 | 26 | .065190 | 6.4665 | 2.430 | 2.491 |
| Indiana | 4,662,498 | 13 | .021761 | 4.6672 | 1.762 | 1.786 |
| Iowa | 2,757,537 | 9 | .011564 | 4.1936 | 1.581 | 1.596 |
| Kansas | 2,178,611 | 7 | .0079966 | 3.6705 | 1.385 | 1.392 |
| Kentucky | 3,038,156 | 9 | .012143 | 3.9968 | 1.509 | 1.521 |
| Louisiana | 3,257,022 | 10 | .013967 | 4.2882 | 1.619 | 1.635 |
| Maine | 969,265 | 4 | .0030605 | 3.1575 | 1.192 | 1.186 |
| Maryland | 3,100,689 | 10 | .013609 | 4.3890 | 1.657 | 1.675 |
| Massachusetts | 5,148,578 | 14 | .024652 | 4.7881 | 1.808 | 1.834 |
| Michigan | 7,823,194 | 21 | .046039 | 5.8849 | 2.223 | 2.262 |
| Minnesota | 3,413,864 | 10 | .014300 | 4.1888 | 1.581 | 1.597 |
| Mississippi | 2,178,141 | 7 | .0079966 | 3.6712 | 1.385 | 1.392 |
| Missouri | 4,319,813 | 12 | .019273 | 4.4615 | 1.683 | 1.710 |
| Montana | 674,767 | 4 | .0025455 | 3.7724 | 1.423 | 1.421 |
| Nebraska | 1,411,330 | 5 | .0045350 | 3.2133 | 1.211 | 1.231 |
| Nevada | 285,278 | 3 | .0012467 | 4.3701 | 1.649 | 1.636 |
| New Hampshire | 606,921 | 4 | .0024238 | 3.9936 | 1.506 | 1.499 |
| New Jersey | 6,066,782 | 17 | .032595 | 5.3727 | 2.026 | 2.063 |
| New Mexico | 951,023 | 4 | .0030342 | 3.1905 | 1.204 | 1.197 |
| New York | 16,782,304 | 43 | .146181 | 8.7104 | 3.287 | 3.312 |
| North Carolina | 4,556,155 | 13 | .021538 | 4.7272 | 1.781 | 1.807 |
| North Dakota | 632,446 | 4 | .0024692 | 3.9024 | 1.472 | 1.468 |
| Ohio | 9,706,397 | 26 | .064013 | 6.5949 | 2.487 | 2.539 |
| Oklahoma | 2,328,284 | 8 | .0094188 | 4.0454 | 1.528 | 1.541 |
| Oregon | 1,768,687 | 6 | .0061779 | 3.4929 | 1.317 | 1.321 |
| Pennsylvania | 11,319,366 | 29 | .078088 | 6.8986 | 2.604 | 2.638 |
| Rhode Island | 859,488 | 4 | .0028828 | 3.3541 | 1.264 | 1.259 |
| South Carolina | 2,382,594 | 8 | .0095422 | 4.0050 | 1.509 | 1.524 |
| South Dakota | 680,514 | 4 | .0025588 | 3.7601 | 1.419 | 1.415 |
| Tennessee | 3,567,089 | 11 | .016093 | 4.5115 | 1.702 | 1.721 |
| Texas | 9,579,677 | 25 | .061003 | 6.3680 | 2.404 | 2.452 |
| Utah | 890,627 | 4 | .0029358 | 3.2963 | 1.245 | 1.237 |
| Vermont | 389,881 | 3 | .0014597 | 3.7440 | 1.415 | 1.400 |
| Virginia | 3,966,949 | 12 | .018454 | 4.6519 | 1.755 | 1.784 |
| Washington | 2,853,214 | 9 | .011754 | 4.1196 | 1.555 | 1.569 |
| West Virginia | 1,860,421 | 7 | .0073532 | 3.9524 | 1.491 | 1.506 |
| Wisconsin | 3,951,777 | 12 | .018415 | 4.6599 | 1.758 | 1.788 |
| Wyoming | 330,066 | 3 | .0013411 | 4.0631 | 1.532 | 1.521 |

**Table 2. (Continued)**

| | Population $m_i$ | Electoral Votes $w_i$ | Total Value $\Psi_i$ | Value Per Inh. $\Psi_i/m_i$ ($\times 10^{-9}$) | Ratio $v_i/v_{\text{d.c.}}$ | Banzhaf Ratio $\beta_i$ |
|---|---|---|---|---|---|---|
| | | 1970 | | | | |
| Alabama | 3,444,165 | 9 | .012003 | 3.4849 | 1.406 | 1.421 |
| Alaska | 302,173 | 3 | .001186 | 3.9253 | 1.584 | 1.582 |
| Arizona | 1,772,482 | 6 | .005740 | 3.2385 | 1.307 | 1.314 |
| Arkansas | 1,923,295 | 6 | .005977 | 3.1079 | 1.254 | 1.261 |
| California | 19,953,134 | 45 | .156584 | 7.8476 | 3.166 | 3.177 |
| Colorado | 2,207,259 | 7 | .007474 | 3.3861 | 1.366 | 1.376 |
| Connecticut | 3,032,217 | 9 | .011271 | 3.7171 | 1.500 | 1.515 |
| Delaware | 548,104 | 3 | .001597 | 2.9131 | 1.175 | 1.175 |
| Dist. of Columbia | 756,510 | 3 | .001875 | 2.4783 | 1.000 | 1.000 |
| Florida | 6,789,443 | 17 | .032070 | 4.7326 | 1.906 | 1.941 |
| Georgia | 4,589,575 | 12 | .018515 | 4.0341 | 1.628 | 1.651 |
| Hawaii | 769,913 | 4 | .002521 | 3.2804 | 1.324 | 1.325 |
| Idaho | 713,008 | 4 | .002428 | 3.4073 | 1.375 | 1.376 |
| Illinois | 11,113,976 | 26 | .063718 | 5.7332 | 2.313 | 2.361 |
| Indiana | 5,193,669 | 13 | .021349 | 4.1106 | 1.659 | 1.684 |
| Iowa | 2,825,041 | 8 | .009662 | 3.4202 | 1.380 | 1.392 |
| Kansas | 2,249,071 | 7 | .007544 | 3.3542 | 1.353 | 1.363 |
| Kentucky | 3,219,311 | 9 | .011609 | 3.6062 | 1.455 | 1.470 |
| Louisiana | 3,643,180 | 10 | .013730 | 3.7686 | 1.521 | 1.538 |
| Maine | 993,663 | 4 | .002863 | 2.8860 | 1.164 | 1.166 |
| Maryland | 3,922,399 | 10 | .014238 | 3.6300 | 1.464 | 1.482 |
| Massachusetts | 5,689,170 | 14 | .024084 | 4.2333 | 1.708 | 1.736 |
| Michigan | 8,875,083 | 21 | .045549 | 5.1322 | 2.071 | 2.113 |
| Minnesota | 3,805,069 | 10 | .014027 | 3.6864 | 1.487 | 1.505 |
| Mississippi | 2,216,912 | 7 | .007490 | 3.3787 | 1.363 | 1.373 |
| Missouri | 4,677,399 | 12 | .018688 | 3.9954 | 1.612 | 1.635 |
| Montana | 694,409 | 4 | .002397 | 3.4516 | 1.393 | 1.394 |
| Nebraska | 1,483,791 | 5 | .004375 | 2.9488 | 1.190 | 1.194 |
| Nevada | 488,738 | 3 | .001508 | 3.0854 | 1.245 | 1.244 |
| New Hampshire | 737,681 | 4 | .002470 | 3.3486 | 1.351 | 1.353 |
| New Jersey | 7,168,164 | 17 | .032935 | 4.5946 | 1.854 | 1.889 |
| New Mexico | 1,016,000 | 4 | .002897 | 2.8516 | 1.151 | 1.153 |
| New York | 18,190,740 | 41 | .134168 | 7.3756 | 2.976 | 3.004 |
| North Carolina | 5,082,059 | 13 | .021122 | 4.1563 | 1.677 | 1.702 |
| North Dakota | 617,761 | 3 | .001695 | 2.7435 | 1.107 | 1.107 |
| Ohio | 10,652,017 | 25 | .059855 | 5.6191 | 2.267 | 2.314 |
| Oklahoma | 2,559,253 | 7 | .008042 | 3.1423 | 1.268 | 1.278 |
| Oregon | 2,091,385 | 5 | .006231 | 2.9793 | 1.202 | 1.209 |
| Pennsylvania | 11,793,909 | 27 | .068299 | 5.7910 | 2.337 | 2.385 |
| Rhode Island | 949,723 | 4 | .002802 | 2.9498 | 1.190 | 1.192 |
| South Carolina | 2,590,516 | 8 | .009257 | 3.5734 | 1.442 | 1.454 |
| South Dakota | 666,257 | 4 | .002347 | 3.5260 | 1.423 | 1.424 |
| Tennessee | 3,924,164 | 10 | .014242 | 3.6292 | 1.464 | 1.482 |
| Texas | 11,196,730 | 26 | .063950 | 5.7115 | 2.305 | 2.352 |
| Utah | 1,059,273 | 4 | .002958 | 2.7924 | 1.127 | 1.129 |
| Vermont | 444,732 | 3 | .001438 | 3.2362 | 1.306 | 1.305 |
| Virginia | 4,648,494 | 12 | .018631 | 4.0080 | 1.617 | 1.640 |
| Washington | 3,409,169 | 9 | .011942 | 3.5030 | 1.413 | 1.428 |
| West Virginia | 1,744,237 | 6 | .005695 | 3.2648 | 1.317 | 1.324 |
| Wisconsin | 4,417,933 | 11 | .016631 | 3.7645 | 1.519 | 1.539 |
| Wyoming | 332,416 | 3 | .001244 | 3.7423 | 1.510 | 1.509 |

$$4.1 \qquad h = f \circ (g_1, \cdots, g_{51})$$

is the composite function. (See Owen[6] for details of this.)

Now, the MLE $f$ was discussed in section 3 above. To construct the MLE $g_j$, we note that $g_j(x_1, \cdots, x_{mj})$ is the probability that the coalition $\bar{S}$ will have more than $m_j/2$ members, given that the several players have probability $x_1, x_2, \cdots, x_{mj}$ of belonging, and that these are independent. We are interested, however, only in values of $g_j$ along the "diagonal" $x_1 = \cdots x_{mj} = t$, and there we find that

$$4.2 \quad g_j(t, t, \cdots, t) = \text{Prob}\left\{Z > \frac{m_j}{2}\right\},$$

where $Z$ is a binomial random variable with parameters $m_j$ and $t$. This can be approximated (with truly negligible error) by a normal variable with mean $tm_j$ and variance $t(1-t)m_j$. Thus,

$$4.3 \quad g_j(t, t, \cdots, t) \cong \Phi\left[(t - \tfrac{1}{2})\sqrt{\frac{m_j}{t(1-t)}}\right],$$

where $\Phi$ is the cumulative normal distribution function.

Now, as is shown in Owen,[7] the value to all voters in the $i$-th state together is given by

$$4.4 \quad \Psi_i = \int_0^1 f_i(x_1, x_2, \cdots, x_{51}) \frac{dx_i}{dt}\, dt,$$

where the integration is taken over the path

$$4.5 \quad x_j(t) = g_j(t, \cdots, t) = \Phi\left[(t - \tfrac{1}{2})\sqrt{\frac{m_j}{t(1-t)}}\right].$$

For the sake of simplicity, it is easier to change the parameter from $t$ to $T$, where

$$4.6 \qquad T = \frac{t - \frac{1}{2}}{\sqrt{t(1-t)}}.$$

Then $T$ varies from $-\infty$ to $+\infty$ as $t$ goes from 0 to 1. Thus we will also have

$$4.7 \quad \Psi_i = \int_{-\infty}^{+\infty} f_i(x_1, \cdots, x_n) \frac{dx_i}{dT}\, dT,$$

where the path of integration is

$$4.8 \qquad x_j(T) = \Phi(T\sqrt{m_j}).$$

It is now possible to approximate $f_i(x_1, \cdots, x_n)$ as in section 3 above; we apply the formulae (3.5), (3.6) and (3.7), as described there. Note that, although (4.7) is formally an improper integral, it is in practice possible to neglect all values of $T$ outside some small interval so that only a finite integration is necessary. Once $\Psi_i$, the value to all voters in the $i$-th state, has been computed, this can be divided by $m_i$ to obtain the value to each individual voter from that state (clearly all voters in the same state have equal powers).

A certain difficulty arises here; namely, that we do not know $m_i$, the number of voters. We know the population, and have an assumption that $m_i$ is proportional to it, but we lack a constant of proportionality. Since, however, we are not so much interested in the actual power of each voter, but only in the disparity of power between voters from different states, the problem is not a serious one. We shall use the population figures for $m_i$; the eventual power obtained per individual can then be divided by the power per individual in the most disadvantaged state (really, the District of Columbia) to obtain power ratios.

Table 2 shows the results of these computations, together with a further approximation obtained by dividing $\mathcal{G}_i$ (as given in Table 1) by $\sqrt{m_i}$. (This is, in effect, the Banzhaf ratio.)

As in any approximative method, one should worry somewhat about the errors introduced by the approximation. Briefly speaking, there are three sources of error here: (a) the error introduced by the approximation in formula (4.3); (b) the error introduced by the approximation in formula (3.7); (c) the error arising from numerical integration.

Of these three, (a) can safely be neglected; $m_i$ is clearly large enough so that the normal approximation to the binomial will be extremely accurate. Similarly, (c) can be made very small if the steps of integration are small enough, as indeed they are here. The main source of error, then, is (b). It is difficult to estimate this error a priori; we might, however, expect that the error here will be approximately the same as that obtained for the state game (section 3). Comparison of the exact figures with the MLE approximations, as seen in Table 1, suggests that the error here is not large either. Thus we feel these results are reasonably accurate.

## 5. Conclusions

Generally speaking, four factors seem to contribute to the disparity of power in presidential elections. Two of these, of course, have always been known. The others have been more or less suspected, but it is only with game-theoretic analysis that they can be measured.

a. The first factor is what might be called the integrity factor, which is best explained by saying that the number of electors from each state must be an integer. The obvious consequence is that there may be states with sizable differences of population but the same number of electors, e.g., Alaska with 302,173 inhabitants and North Dakota with 617,761. Both have one U.S. Representative, and hence both have 3 electors. This factor clearly benefits the voters from states whose population is low among those with equal numbers of U.S. Representatives.

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[6] Owen, "Multilinear Extensions."

[7] Owen, "Multilinear Extensions."

b. The second factor might be called the Senate factor, and is explained by saying that the number of electors is equal to a state's total congressional representation. Now the state's delegation to the House is (more or less) proportional to population, but all states have two senators. Thus, this factor tends to favor states with low population.

c. The third factor, which was first noticed by Shapley and Mann (see note 5), might be called the unit rule factor. Essentially, we see in Table 1 that, if the values to the states in the state game are multiplied by 538, the power for the larger states is slightly more than their electoral vote; that for the smaller states is slightly less than their electoral vote. This factor favors the large states.

d. The fourth factor can best be described as the composition factor. This can best be seen mathematically from formula (4.7), where the factor $dx_i/dT$ appears. From (4.8), we will have

$$\frac{dx_i}{dT} = \sqrt{\frac{m_j}{2\pi}}\, e^{-\frac{1}{2}m_j T^2}, \tag{5.1}$$

which, for small values of $T$, is roughly proportional to $\sqrt{m_j}$. (For larger values of $T$, of course, the exponential term decreases rapidly as $m_j$ increases.) Since most of the integral (4.7) will be concentrated near $T=0$, it follows that there will be an additional factor, not quite equal to $\sqrt{m_j}$, favoring the larger states.

Of these four factors, it is clear that (a) and (b) were expected by the original framers of the Constitution. Factor (a) was probably considered minor, a necessary evil to contend with, while (b) was part of a compromise which made the smaller states more willing to join the union. The other two factors, however, are probably quite unexpected. Factor (c) was certainly not expected by the Constitution's framers, since the present method of pledged electors was not then foreseen; in any case it is minor, as Table 1 shows. The strongest factor, from Table 2, is clearly factor (d). Somehow, people have been aware of this factor's existence, as evidenced by the traditional rule which says that the more populous states are usually "swing" states. It is only with game-theoretic analysis, however, that the extent of this factor has been measured, first by Banzhaf,[8] and now in this paper.

In closing, it might be worthwhile to comment on a recent controversy over allocation of resources in a presidential election.

In one paper[9], Brams and Davis recommend a "3/2 power rule" for allocation, and suggest that this is empirically tested by strategies actually used in the last four presidential elections. In another paper[10], Colantoni, Levesque and Ordeshook disagree.

It should be pointed out that our model is quite different, treating the election as an $n$-person game among the voters, while the authors in both the above papers treat an election as a two-person game between the candidates. Moreover, it is dangerous to try to find empirical validation of a non-obvious and complicated mathematical model. Nonetheless, it is interesting to note that the Shapley value as here obtained is very similar to the allocation recommended by Brams and Davis.

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[8] Banzhaf, "One Man, 3.312 Votes."

[9] Steven J. Brams and Morton D. Davis, "The 3/2's Rule in Presidential Campaigning," *American Political Science Review*, 68 (1974), 113–134.

[10] Claude S. Colantoni, Terrence J. Levesque, and Peter C. Ordeshook, "Campaign Resource Allocations Under the Electoral College," *American Political Science Review*, 69 (1975), 141–154; followed by a "Comment" from Brams and Davis, 155–156, and a "Rejoinder" by Colantoni et al, 157–161.

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# Vote Trading and the Voting Paradox: A Proof of Logical Equivalence*

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In a recent paper concerned with legislative vote trading, Riker and Brams demonstrated "... the paradox of logrolling: that rational trades by all members [may] make everyone worse off."[1] In discussing this effect the authors note that "There seems ... to be an inherent disequilibrium in vote trading which is occasioned by the assumption of individual rationality."[2] This instability associated with vote trading has been noted by a number of others. R. E. Park, considering the set of possible legislative outcomes, proved a theorem which states:

> If the introduction of vote trading is 'beneficial' in the sense that some majority set can, with vote trading, improve the payoffs to all of its members over those without vote trading, there is no stable outcome with vote trading."[3]

Tullock has also indicated that instability exists at least in the case of "implicit" logrolling (formation by candidates or parties of platforms: "... complex mixes of different measures").[4] He notes, however, that "explicit" logrolling, ordinary exchanges of votes among legislators, is stable.[5] Further, both Tullock and Coleman in different articles have indicated that logrolling, by taking intensities of preference into account, eliminates irrationality of the sort described by Arrow.[6]

In this paper I begin with an example of a vote-trading situation taken from Riker and Brams and demonstrate that an endless sequence of trades can be expected under conditions of individual rationality. Then the utility numbers are replaced with ordinal notation, and a proof is given which demonstrates that the individual preference patterns required for vote trading are necessary and sufficient conditions for the voting paradox: that vote trading and the voting paradox are logically equivalent.[7] Following this result, a few concluding remarks are directed to the previous considerations of logrolling.

### The Instability of Vote Trading

Riker and Brams have constructed a model which specifies the conditions which must exist if rational individuals are to exchange votes. These may be summarized briefly as follows:

1. "... the traders must be on opposite sides on two motions."
2. Each trader must be in a majority on one motion and a minority on the other.
3. Each trader's salience must be higher on his minority motion than his majority motion.
4. Each trader must be pivotal in his majority.[8]

These conditions must be met if trading is to

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[1] William H. Riker and Steven J. Brams, "The Paradox of Vote Trading," *American Political Science Review*, 67 (December, 1973), 1241.

[2] Riker and Brams, p. 1246.

[3] R. E. Park, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, 57 (December, 1967), 1304.

[4] Gordon Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, 60 (June, 1970), 422.

[5] *Ibid.*, p. 424.

[6] Tullock makes the point in an appendix to James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan, 1962), p. 332, as does James Coleman, "The Possibility of a Social Welfare Function," *American Economic Review*, 56 (1966), 1105–22. These remarks are addressed to Kenneth J. Arrow, *Social Choice and Individual Values*, Cowles Foundation Monograph No. 12, 2nd ed. (New York: Wiley, 1965), pp. 1–33.

[7] Since the completion of this work, several other papers dealing with the subject have come to my attention. Peter Bernholz, "Logrolling, Arrow Paradox and Cyclical Majorities," *Public Choice*, 16 (Summer, 1973), 87–102 and "Logrolling, Arrow Paradox and Decision Rules: A Generalization," *Kyklos*, 27 (November, 1973), 49–62, has demonstrated that vote trading implies the voting paradox. He does not prove that they are logically equivalent. Also Joseph Kadane, "On Division of the Question," *Public Choice*, 13 (Fall, 1972) 47–54, and Nicholas R. Miller, "Logrolling and the Arrow Paradox: A Note," unpublished manuscript (December, 1973) have addressed the equivalence by considering the situation where there is no undominated platform confronting a set of voters. This implies that members will have an incentive to trade votes, and that a cyclical majority is present.

Joe A. Oppenheimer, in "Relating Coalitions of Minorities to the Voters' Paradox or Putting the Fly in the Democratic Pie," a paper delivered at the Annual Meeting at the South West Political Science Association Meeting, San Antonio, Texas, March 30—April 1, 1972, has shown that in legislatures or elections "an efficacious coalition of minorities can exist if, and only if, there is an underlying voters' paradox" (p. 5). Oppenheimer's logic is similar to that presented here; the structure of the two arguments, however, is quite different. If the voting paradox is equivalent to both the "coalition of minorities" and logrolling, it follows logically that these two are one and the same.

[8] Riker and Brams, pp. 1236–40.

**Table 1. Utilities of Members for Motions on X and Y**

| Member | Preferred Outcome | Motion $x$ | | Preferred Outcome | Motion $y$ | |
|---|---|---|---|---|---|---|
| | | $u(x)$ | $u(\bar{x})$ | | $u(y)$ | $u(\bar{y})$ |
| $m_1$ | $x$ | 1 | $-2$ | $y$ | 1 | $-2$ |
| $m_2$ | $x$ | 1 | $-1$ | $\bar{y}$ | $-2$ | 2 |
| $m_3$ | $\bar{x}$ | $-2$ | 2 | $y$ | 1 | $-1$ |

occur, and they form the basis for several examples constructed by the authors to illustrate the process. The examples provide the starting point for this analysis, and though they are limited to a three member legislature confronting six issues, or for purposes of this paper only two issues, it is anticipated that they are generalizable. As Riker and Brams point out, "... the three member example can, of course, be embedded in any legislature; and of course, the three members can be three factions."[9]

Consider a legislature with three members ($m_1$, $m_2$, $m_3$) deciding two issues ($X$ and $Y$). The situation is summarized in Table 1 where the numbers represent utilities which individual members attribute to the possible collective choices on motions $x$ and $y$.[10] These have obviously been chosen in such a fashion that the members will be able to make individually beneficial exchanges of votes; as such, they are consistent with the four conditions for vote trading stated above.

The salience, $s$, for member $i$ on issue $X$ is defined as the absolute utility difference between the possible outcomes $x$ and $\bar{x}$.[11] For example, the salience for member $m_2$ is

$$s_2(x) = |u_2(x) - u_2(\bar{x})| = |1 - (-1)| = 2$$

The same calculation for issue $Y$ gives $s_2(y) = 4$ and illustrates the meaning of "higher" salience referred to in Condition 3. The voting outcomes include the combinations of possible decisions on individual motions and may be given as follows: $\{x \wedge y,\ x \wedge \bar{y},\ \bar{x} \wedge y,\ \bar{x} \wedge \bar{y}\}$. The listing implies that the motions are considered jointly by the members. This is intended since it is clearly required for a rational exchange of votes.

Consider first the outcome resulting from sincere or naive voting; that is, "... voting in accord with one's true tastes ..." on each successive motion.[12] From the utilities or preferences of each member (Table 1) it is apparent that in the absence of vote trading the outcome will be $x \wedge y$ ($x$ is preferred to $\bar{x}$ by $m_1$ and $m_2$, and $y$ to $\bar{y}$ by $m_1$ and $m_3$). The members' utilities for $x$, $y$ and $x \wedge y$ are given in Section A of Table 2, which also lists all the other possible outcomes and which illustrates the process of trading and its associated instability.

Now when $m_2$ and $m_3$ are confronted with the prospective outcome $x \wedge y$, vote-trading models assert that they will realize than an exchange of votes will be mutually beneficial. Member $m_2$ will offer to vote against his own interest on his less salient issue $X$ ($s_2(x) = 2$, $s_2(y) = 4$) thereby insuring $m_3$ a favorable outcome $\bar{x}$ on his more salient issue ($s_3(x) = 4$, $s_3(y) = 2$) if in return $m_3$ will vote against his own interest on his less salient issue $Y$ thereby insuring $m_2$ a favorable outcome $\bar{y}$ on his more salient issue. If agreement is reached, a new outcome, $\bar{x} \wedge \bar{y}$, is expected or actually realized if the vote is taken.[13] This is shown in Section B of Table 2 where the members' payoffs are given for $\bar{x} \wedge \bar{y}$. The column headed "total $u$" gives the sum of the component utilities, while "$\Delta u$" refers to the change in total utility from the previous "naive" state. Note that in Section B both $m_2$ and $m_3$ have experienced a positive increase in total utility relative to the naive voting outcome $x \wedge y$.

For purposes of this example, assume that the vote is not taken—that the members after the first trade are confronted by a new *potential* outcome $\bar{x} \wedge \bar{y}$. In this case, one of two further exchanges may take place: $m_1$ will either approach $m_2$ or $m_3$. It does not matter which occurs first since either may make a mutually beneficial agreement with $m_1$. Arbitrarily beginning with $m_2$, one can see that it is rational for $m_1$ to switch his vote from $y$ to $\bar{y}$, which $m_2$ favors, if $m_2$ will break his agreement with $m_3$ and change his vote from $\bar{x}$ to $x$, which both $m_1$ and $m_2$ prefer.[14] The result is the

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[9] *Ibid.*, p. 1241.

[10] Except for minor notational differences this is taken directly from Table 5, Riker and Brams, p. 1241.

[11] This concept is developed in some detail in Riker and Brams, p. 1239.

[12] Riker and Brams, p. 1237.

[13] The primary concern is not the actual occurrence of a particular outcome following a trade. The important point is that if either votes or intentions to vote are exchanged, there is no equilibrium.

[14] This trade sounds somewhat strange since ordinarily one member does not promise another a vote on an issue which the latter has already won. What this does, of course, is enable $m_2$ to break his previous agreement with $m_3$, who has promised to vote $\bar{y}$, and be assured that $\bar{y}$ still wins. The reason for using this device is that it allows the number of issues in the example to be kept to a minimum.

**Table 2. The Trading Cycle**

A. Naive voting

| | Members' Utilities for Outcome $x \wedge y$ | | | |
|---|---|---|---|---|
| | $u(x)$ | $u(y)$ | Total $u$ | $\Delta u$ |
| $m_1$ | 1 | 1 | 2 | |
| $m_2$ | 1 | $-2$ | $-1$ | |
| $m_3$ | $-2$ | 1 | $-1$ | |

B. Trade between $m_2$ and $m_3$

| | Members' Utilities for Outcome $\bar{x} \wedge \bar{y}$ | | | |
|---|---|---|---|---|
| | $u(\bar{x})$ | $u(\bar{y})$ | Total $u$ | $\Delta u$ |
| $m_1$ | $-2$ | $-2$ | $-4$ | $-6$ |
| $m_2$ | $-1$ | 2 | 1 | 2 |
| $m_3$ | 2 | $-1$ | 1 | 2 |

C. Trade between $m_1$ and $m_2$

| | Members' Utilities for Outcome $x \wedge \bar{y}$ | | | |
|---|---|---|---|---|
| | $u(x)$ | $u(\bar{y})$ | Total $u$ | $\Delta u$ |
| $m_1$ | 1 | $-2$ | $-1$ | 3 |
| $m_2$ | 1 | 2 | 3 | 2 |
| $m_3$ | $-2$ | $-1$ | $-3$ | $-4$ |

D. Trade between $m_1$ and $m_3$

| | Members' Utilities for Outcome $x \wedge y$ | | | |
|---|---|---|---|---|
| | $u(x)$ | $u(y)$ | Total $u$ | $\Delta u$ |
| $m_1$ | 1 | 1 | 2 | 3 |
| $m_2$ | 1 | $-2$ | $-1$ | $-4$ |
| $m_3$ | $-2$ | 1 | $-1$ | 2 |

potential outcome $x \wedge \bar{y}$, with both $m_1$ and $m_2$ unambiguously better off (see Section C, Table 2, where both $m_1$ and $m_2$ experience a positive $\Delta u$ over the previous state).

The next step is clear: $m_3$ approaches $m_1$ with a promise to switch his vote from $\bar{x}$ to $x$, which $m_1$ favors, if $m_1$ will break his agreement with $m_2$ and change his vote from $\bar{y}$ to $y$, which both $m_1$ and $m_3$ prefer to $\bar{y}$. Since it is rational for $m_1$ and $m_3$ to make such a trade, the outcome will be $x \wedge y$, which is the same as would occur with naive voting. Now the process is ready to begin again.[15]

While this example illustrates the cyclical nature of vote trading, it does not establish that cycling is logically associated with vote trading; the utility numbers could constitute a special case. To eliminate this possibility, the members' preferences can be expressed in ordinal-utility terms subject only to such constraints as are required for individually rational trades to occur (i.e. the preferences are consistent with the four conditions stated above).

Consider a situation comparable to that portrayed in Table 1 where three members are deciding issues $X$ and $Y$. There is for each member an ordinal preference on each issue and a salience relation between the issues (indifference is not of concern since vote trading implies that the members express preferences on each motion). If, for example, $m_1$ preferred $x$ to $\bar{x}$ it would be written $u_1(x) > u_1(\bar{x})$; if motion $y$ relative to $\bar{y}$ were more important to him than motion $x$ relative to $\bar{x}$, it would be written $s_1(y) > s_1(x)$. In setting forth the members' preferences as required for vote trading, $m_1$ is designated a trader and a set of preference relations is arbitrarily selected for him. Following that, all of the remaining preferences for the second trader ($m_2$) and the third member (who is the innocent bystander) are fixed by the vote-trading Conditions 1–4. There are of course eight possible combinations of preference and salience relations for $m_1$; however, the different possibilities simply amount to differences in labeling, and the deductions which follow from one configuration are true for all the others.

Assume that trader $m_1$ has the following preferences:

$$u_1(x) > u_1(\bar{x})\text{—given,}$$
$$u_1(y) > u_1(\bar{y})\text{—given,}$$
$$s_1(y) > s_1(x)\text{—given.}$$

Then if trade is to take place, the second trader, $m_2$, must have a complimentary set of preferences:

$$u_2(\bar{x}) > u_2(x)\text{—Condition 1,}$$
$$u_2(\bar{y}) > u_2(y)\text{—Condition 1,}$$
$$s_2(x) > s_2(y)\text{—Condition 3.}$$

Further, the bystander, $m_3$, plays a crucial role in filling out the appropriate majorities:

$$u_3(x) > u_3(\bar{x})\text{—Conditions 2 and 3,}$$
$$u_3(\bar{y}) > u_3(y)\text{—Conditions 2 and 3,}$$
$$s_3(x) = s_3(y)\text{—arbitrary.}$$

(For $m_3$, the relative salience of $x$ and $y$ has no effect on the steps which follow. By setting them

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The question of the effect of broken agreements on continued vote trading is not a primary concern given the perspective of this paper. The matter is discussed by Dennis C. Mueller, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, 57 (December, 1967), 1304–11, and by James Coleman, "The Possibility of a Social Welfare Function: Reply," *American Economic Review*, 57 (December, 1967), 1311–17 in a response to R. E. Park, "Comment."

[15] The purpose of this argument is not to suggest that whenever votes are exchanged in the real world a frantic process of cycling commences. Rather it is to show that when votes can be exchanged, instability follows logically, whether or not it takes place in practice.

equal it is possible to make the most general statement of the argument.)

Once the members' preference and salience relations are established, one may determine the preference orderings for each over the set of outcomes:

*Member 1 (Trader)*

1. $u_1(x) > u_1(\bar{x})$, $u_1(y) > u_1(\bar{y})$ and $s_1(y) > s_1(x)$

$$\rightarrow u_1(x \wedge y) > u_1(\bar{x} \wedge y) > u_1(x \wedge \bar{y}) > u_1(\bar{x} \wedge \bar{y})$$

*Member 2 (Trader)*

2. $u_2(\bar{x}) > u_2(x)$, $u_2(\bar{y}) > u_2(y)$ and $s_2(x) > s_2(y)$

$$\rightarrow u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y) > u_2(x \wedge \bar{y}) > u_2(x \wedge y)$$

*Member 3 (Bystander)*

3. $u_3(x) > u_3(\bar{x})$, $u_3(\bar{y}) > u_3(y)$ and $s_3(y) = s_3(x)$

$$\rightarrow u_3(x \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)$$

or

$$u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge y)$$

Implications 1–3 state the individual orderings over the outcome set (proofs are given in the Appendix). Since there are only four possible outcomes, the notation can be simplified before demonstrating the existence of cyclical majorities:

Let

$$A = u_i(x \wedge y)$$
$$B = u_i(x \wedge \bar{y})$$
$$C = u_i(\bar{x} \wedge y)$$
$$D = u_i(\bar{x} \wedge \bar{y})$$

It may be recalled that in the numerical example, outcome $\bar{x} \wedge y$ was not involved. This is because, after the first trade, $m_1$ was arbitrarily selected to approach $m_2$ before $m_3$ (see Section C of Table 2). Had the opposite choice been made, $\bar{x} \wedge y$ would have appeared in the cycle instead of $x \wedge \bar{y}$; however, in any complete trading cycle, only one of the two will be present. In the ordinal utility example just above, either $x \wedge y$ or $\bar{x} \wedge \bar{y}$ will be present and these correspond to $A$ and $D$ in the simplified notation. Thus from Implications 1–3 the members' preference orderings are listed and the resulting intransitivities demonstrated (Table 3), dealing first with one (D) and then the other (A). Next for the sake of completeness, intransitivity is shown to exist over the entire set of outcomes. It is assumed that all pair-wise combinations of outcomes are decided by majority rule.

In considering all four outcomes, there are two possible cases: $m_3$ may either express a preference for $A$ as compared to $D$ or vice versa. (In effect, the equal salience assumption for $m_3$, which leads to his indifference between $A$ and $D$ as expressed in Implication 3, is now discarded.) Table 4 presents both possibilities and demonstrates that the instability is unaffected.

**Table 3. Social Choice Under Vote-Trading Conditions**

| Preference Orders Excluding $A$ | Preference Orders Excluding $D$ |
|---|---|
| $m_1$ $C > B > D$ | $m_1$ $A > C > B$ |
| $m_2$ $D > C > B$ | $m_2$ $C > B > A$ |
| $m_3$ $B > D > C$ | $m_3$ $B > A > C$ |
| Social Choice: | Social Choice: |
| $C > B > D > C$ | $A > C > B > A$ |

**Table 4. Social Choice Under Vote-Trading Conditions**

| Member $m_3$ Prefers $A$ to $D$ | Member $m_3$ Prefers $D$ to $A$ |
|---|---|
| $m_1$ $A > C > B > D$ | $m_1$ $A > C > B > D$ |
| $m_2$ $D > C > B > A$ | $m_2$ $D > C > B > A$ |
| $m_3$ $B > A > D > C$ | $m_3$ $B > D > A > C$ |
| Social Choice: | Social Choice: |
| $D > C > B > A > D$ | $B > D > A > C > B$ |

It has been shown that *if* three members meet the conditions for vote trading, the cyclical majority follows. Still, this falls short of proving the logical equivalence of the two processes since it has not been demonstrated that the cyclical majority occurs *only if* the conditions for vote trading are present. The latter statement is in fact true. It can be proved by selecting preference configurations which generate cyclical voting and showing that when these preferences exist, the conditions for trade follow.

In the set of 216 possible 3×3 preference matrices, twelve result in the cyclical majority.[16] Two of these, listed in Table 5 involve different preference patterns. The remaining cyclical preference matrices are formed by the five alternative

**Table 5. Cyclical Preference Orders**

| | I | II |
|---|---|---|
| $m_1$ | $S > R > T$ | $S > T > R$ |
| $m_2$ | $T > S > R$ | $T > R > S$ |
| $m_3$ | $R > T > S$ | $R > S > T$ |

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[16] The preference configurations which lead to the cyclical majority are discussed in William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, New Jersey: Prentice-Hall, 1973), pp. 94–96.

permutations of the members for each preference pattern. In this case, $R$, $S$ and $T$ may be considered either a set of complex outcomes such as those discussed above (e.g. $x \wedge \bar{y}$), or simply a set of three competing alternatives in the context commonly used to demonstrate the voting paradox. The results are applicable regardless of the interpretation. Only the first of the two matrices is considered, since the second produces the same effect. In the matrix, the notation $R > S$ indicates that the specified member prefers $R$ to $S$.

First, it may be stated without proof that orderings of the sort indicated for the three members in Table 5 are required to produce cycling. Simply, each outcome or motion must be given a different rank by each member. Any deviation from this pattern results in a majority winner.[17]

Next, examination of Matrix I in Table 5 indicates that the conditions for trade are fulfilled.

1. *Traders must be on opposite sides of two motions.* Arbitrarily selecting $m_1$ and $m_2$ as traders, the preference orders show that they are on opposite sides on $R$ vs. $T$ and on $S$ vs. $T$.
2. *Each trader must be in a majority on one motion and a minority on the other.* Member $m_1$ is in a majority on $R$ vs. $T$ and a minority on $S$ vs. $T$ while $m_2$ is in a majority on $S$ vs. $T$ and a minority on $R$ vs. $T$.
3. *Each trader's salience must be higher on his minority motion than his majority motion.* Member $m_1$'s salience is higher on $S$ vs. $T$ than $R$ vs. $T$, while $m_2$'s salience is higher on $R$ vs. $T$ than $S$ vs. $T$.
4. *Each trader must be pivotal in his majority.* This is implicit in Conditions 1 and 2 where there are only three members.

If the conditions for trade are met, exchanges of votes presumably follow. Examination of the members' preferences in Table 5 demonstrates that this is indeed the case. Despite the fact that the final result of the voting is in doubt due to the cyclical majority, members may at least improve their chances with respect to the individual pairwise decisions. That is, $m_1$ would be willing to disregard his preference order, voting for $T$ against $R$, if $m_2$ will in turn vote for $S$ against $T$. This results in the set of "apparent" preferences shown in Table 6 where the effect of the trade is indicated by the balloons. The exchange would result in a victory for $m_1$, on $S$ vs. $T$ and for $m_2$ on $R$ vs. $T$, while the collective choice would be $S$.

**Table 6. Members' Apparent Preferences After Initial Trade**

$m_1\ S > \boxed{T > R}$

$m_2\ \boxed{S > T} > R$

$m_3\ R > T > S$

Looking back to the original orderings (the "real" preferences), one notes that both $m_2$ and $m_3$ prefer $T$ to $S$. Therefore $m_2$ would be willing to break his agreement with $m_1$ and vote according to his "real" preference order if in return, $m_3$ will vote for $T$ against $R$. This leads to the set of "apparent" preferences shown in Table 7 with the trade again indicated by the balloon. Now $T$ would be the collective choice except for the fact that, referring once more to the original preferences, $m_1$ and $m_3$ both prefer $R$ to $T$. This time, $m_3$ would break his agreement with $m_2$ in return for $m_1$'s promise to vote for $R$ against $S$ and $R$ would be the prospective winner.

**Table 7. Members' Apparent Preferences After Second Trade**

$m_1\ S > R > T$

$m_2\ T > S > R$

$m_3\ \boxed{T > R} > S$

Obviously the example can be carried on indefinitely. The important point, however, is that the conditions for trade follow from the preference configurations required to produce the cyclical majority. Thus the proof is completed showing that *if* and *only if* three members meet the conditions for vote trading, the cyclical majority follows. In effect, the two processes are logically equivalent.

## Conclusion

These results indicate that assertions which associate instability with logrolling are well founded. Park's theorem is clearly confirmed and extended here by elucidating the relationship between individual preferences and the cyclical social choice.

The equivalence of the two processes indicates that theorems dealing with either one should apply to the other. For example, Kenneth May has shown that consistent majority decisions are possible if members' choices are limited to a single pair of alternatives.[18] This result extended to vote trading yields the obvious conclusion that members confronting a single pair of alternatives will have no incentive to trade votes. Elsewhere Sen has demonstrated that if members have Value-Restricted Preferences, majority voting will produce consistent social choices.[19] From this it may be inferred that Value-Restricted Preferences will eliminate vote trading among rational legislators. For a third example, Duncan Black has proven that if members' preferences are single-peaked, cyclical majorities will not occur.[20] By the equivalence demonstrated here, one may conclude that single-peaked preferences will likewise preclude the possibility of a mutually beneficial exchange of votes. While none of these theorems provides a practical means for eliminating the cyclical majority, they do elucidate the problem. The same is obviously true in respect to the elimination of vote trading.

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[17] Riker and Ordeshook, pp. 100–109.

[18] Kenneth O. May, "A Set of Independent Necessary and Sufficient Conditions for Simple Majority Decision," *Econometrica*, 20 (October, 1952), 680–684.

The arguments by Tullock and Coleman asserting that logrolling may invalidate Arrow's General Possibility Theorem are somewhat less obvious. Their contention is based on the belief, encouraged by Arrow himself, that his Condition III does not admit voting systems which allow for the expression of intensities of preference, and hence, would exclude decisions based on logrolling. Plott has shown in a recent paper that this is not correct.[21] Further, his proper identification of relevant alternatives as those which constitute the "feasible" set means that vote trading requires changes in apparent preferences over *relevant* rather than *irrelevant* alternatives and, therefore, Condition III does not apply at all. In fact, vote trading would eliminate the paradox only if it could be shown to lead to Value-Restricted Preferences. The proof given above definitely indicates that does not occur.

A major point in Oppenheimer's paper is that the occurrence of the voting paradox is not the rarity some have suggested.[22] Rather than citing examples of it, however, he shows that the paradox inevitably underlies the formation of successful coalitions of minorities. As a result a whole class of events may be taken as indication of the existence of potential voting irrationality. The same may be said for the equivalence demonstrated here. To the extent that vote trading is prevalent in a legislature, it is an indication that the voting paradox may be a far more ubiquitous and troublesome phenomenon than political scientists have thought heretofore.

Finally, regarding Riker and Brams, there is nothing in this work to contradict or limit the importance of the paradox of vote trading. If anything the *a priori* futility of exchanging votes is emphasized; a legislator is unequivocally trapped between the prospect of a never-ending series of trades, broken agreements and legislative uncertainty, and the prospect of even worse material losses if he refuses to participate.[23]

## Appendix

Purpose: To show that the individual members' preferences on issues $X$ and $Y$ lead to the ordering of social states given in Implications 1–3.

I. For member $m_1$, the givens are $u_1(x)>u_1(\tilde{x})$, $u_1(y)>u_1(\tilde{y})$ and $s_1(y)>s_1(x)$. From these it must be proved that $u_1(x\wedge y)>u_1(\tilde{x}\wedge y)>u_1(x\wedge\tilde{y})>u_1(\tilde{x}\wedge\tilde{y})$. This requires the demonstration of six preference relations: that $u_1(x\wedge y)$ is preferred to all of the others, that $u_1(\tilde{x}\wedge y)$ is preferred to $u_1(x\wedge\tilde{y})$ and $u_1(\tilde{x}\wedge\tilde{y})$, and that $u_1(x\wedge\tilde{y})$ is preferred to $u_1(\tilde{x}\wedge\tilde{y})$. In so doing additivity is assumed; that for any motions $p$ and $q$, $u_i(p)+u_i(q)=u_i(p\wedge q)$.

The preference relations may be demonstrated as follows:

a. $u_1(x\wedge y)>u_1(\tilde{x}\wedge y)$, $u_1(x\wedge\tilde{y})$, $u_1(\tilde{x}\wedge\tilde{y})$
  i. If $u_1(x)>u_1(\tilde{x})$, then $\underline{u_1(x\wedge y)>u_1(\tilde{x}\wedge y)}$
  ii. If $u_1(y)>u_1(\tilde{y})$, then $\underline{u_1(x\wedge y)>u_1(x\wedge\tilde{y})}$
  iii. If $u_1(x)>u_1(\tilde{x})$ and $u_1(y)>u_1(\tilde{y})$, then $\underline{u_1(x\wedge y)>u_1(\tilde{x}\wedge\tilde{y})}$

b. $u_1(\tilde{x}\wedge y)>u_1(x\wedge\tilde{y})$, $u_1(\tilde{x}\wedge\tilde{y})$
  iv. If $s_1(y)>s_1(x)$, $u_1(x)>u_1(\tilde{x})$ and $u_1(y)>u_1(\tilde{y})$, then $u_1(y)-u_1(\tilde{y})>u_1(x)-u_1(\tilde{x})$, and then $\underline{u_1(\tilde{x}\wedge y)>u_1(x\wedge\tilde{y})}$
  v. If $u_1(y)>u_1(\tilde{y})$, then $\underline{u_1(\tilde{x}\wedge y)>u_1(\tilde{x}\wedge\tilde{y})}$

c. $u_1(x\wedge\tilde{y})>u_1(\tilde{x}\wedge\tilde{y})$
  vi. If $u_1(x)>u_1(\tilde{x})$, then $\underline{u_1(x\wedge\tilde{y})>u_1(\tilde{x}\wedge\tilde{y})}$

Statement i–vi give the six relations required to establish $m_1$'s ordering of the social states.

II. For member $m_2$, the givens are $u_2(\tilde{x})>u_2(x)$, $u_2(\tilde{y})>u_2(y)$, and $s_2(x)>s_2(y)$. From these it must be proved that $u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y) > u_2(x \wedge y)$. Again six preference relations will be demonstrated.

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[19] Amartya K. Sen, "A Possibility Theorem on Majority Decisions," *Econometrica,* 34 (April, 1966), 491–99, has shown that the cyclical majority can be avoided by assuming Value-Restricted Preferences: ". . . a set of individual preferences over a triple of alternatives such that there exist one alternative and one value with the characteristic that the alternative never has that value in any individual's preference ordering . . ." This is the opposite of the condition stated above to *insure* cycling in the three member—three alternative case, that ". . . each outcome must be given a different rank by each member."

[20] Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1958) pp. 1–51. The condition of single-peakedness assumes that all members' preferences over a set of motions can be ordered on a single dimension such that each preference function ". . . changes its direction at most once, from up to down."

[21] Charles R. Plott, "Rationality and Relevance in Social Choice Theory," Social Science Working Paper No. 5 (California Institute of Technology, August, 1971), pp. 10–13. discusses Condition III and the example from Arrow, pp. 26–28.

[22] Oppenheimer, "Relating Coalitions," pp. 5–6.

[23] Riker and Brams, p. 1242, show that the worst a member can do occurs when others trade and he refuses to join in.

a. $u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y), u_2(x \wedge \bar{y}), u_2(x \wedge y)$
  i. If $u_2(\bar{y}) > u_2(y)$, then $\underline{u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y)}$
  ii. If $u_2(\bar{x}) > u_2(x)$, then $\underline{u_2(\bar{x} \wedge \bar{y}) > u_2(x \wedge \bar{y})}$
  iii. If $u_2(\bar{y}) > u_2(y)$ and $u_2(\bar{x}) > u_2(x)$, then $\underline{u_2(\bar{x} \wedge \bar{y}) > u_2(x \wedge y)}$
b. $u_2(\bar{x} \wedge y) > u_2(x \wedge \bar{y}), u_2(x \wedge y)$
  iv. If $s_2(x) > s_2(y)$, $u_2(\bar{x}) > u_2(x)$ and $u_2(\bar{y}) > u_2(y)$, then $u_2(\bar{x}) - u_2(x) > u_2(\bar{y}) - u_2(y)$, and then $\underline{u_2(\bar{x} \wedge y) > u_2(x > \bar{y})}$
  v. If $u_2(\bar{x}) > u_2(x)$, then $\underline{u_2(\bar{x} \wedge y) > u_2(x \wedge y)}$
c. $u_2(x \wedge \bar{y}) > u_2(x \wedge y)$
  vi. If $u_2(\bar{y}) > u_2(y)$, then $\underline{u_2(x \wedge \bar{y}) > u_2(x \wedge y)}$

Statements i–vi give the six relations required to establish $m_2$'s ordering of the social states.

III. For member $m_3$, the givens are $u_3(x) > u_3(\bar{x})$, $u_3(\bar{y}) > u_3(y)$ and $s_3(y) = s_3(x)$. From these it must be proved that $u_3(x \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)$ or $u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge y)$. Since $u_3(x \wedge y) \gtrless u_3(\bar{x} \wedge \bar{y})$ only five preference relations need be demonstrated.

a. $u_3(x \wedge \bar{y}) > u_3(x \wedge y)$, $u_3(\bar{x} \wedge \bar{y})$, $u_3(\bar{x} \wedge y)$
  i. If $u_3(\bar{y}) > u_3(y)$, then $\underline{u_3(x \wedge \bar{y}) > u_3(x \wedge y)}$
  ii. If $u_3(x) > u_3(\bar{x})$, then $\underline{u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge \bar{y})}$
  iii. If $u_3(\bar{y}) > u_3(y)$ and $u_3(x) > u_3(\bar{x})$, then $\underline{u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge y)}$
b. $u_3(x \wedge y) > u_3(\bar{x} \wedge y)$
  iv. If $u_3(x) > u_3(\bar{x})$, then $\underline{u_3(x \wedge y) > u_3(\bar{x} \wedge y)}$
c. $u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)$
  v. If $u_3(\bar{y}) > u_3(y)$, then $\underline{u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)}$

Statements i–v give the five relations required to establish $m_3$'s ordering of the social states. This completes the proof.

# Logrolling and the Paradox of Voting: Are They Really Logically Equivalent? A Comment

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In his paper "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence" David Koehler[1] claims to have proved the equivalence of logrolling and the paradox of voting. Unfortunately, the proofs given by Koehler do not have the general validity he intends them to have. Indeed, it is easy to show that logrolling does not necessarily imply the paradox of voting, nor does the paradox of voting imply the existence of logrolling situations.

I begin by first demonstrating with the help of a counter-example that certain logrolling situations do not imply the paradox of voting. This is not very surprising, since to prove that logrolling implies the paradox, it has been necessary to assume the absence of a certain kind of complementarity among issues.[2] In the counter-example a group of three voters, $V_1$, $V_2$ and $V_3$, has to decide two issues $x$ and $y$. The two issues each contain two alternatives, namely, $x_1$, $x_2$ and $y_1$, $y_2$. In Matrix 1 the utility payoffs for voters $V_1$, $V_2$ and $V_3$ are given for the four possible outcomes $(x_1, y_1)$, $(x_1, y_2)$, $(x_2, y_1)$ and $(x_2, y_2)$.

*Matrix 1*

| Moves available to all voters | $x_1$ | $x_2$ |
|---|---|---|
| $y_1$ | 8, 8, 8 | 2, 10, 2 |
| $y_2$ | 6, 2, 10 | 10, 6, 6 |

With simple majority voting $x_2$ and $y_2$ will receive a majority of votes, so that $(x_2, y_2)$ will be the outcome selected by the group. This result follows, because $x_1$ is dominated by $x_2$ for $V_2$, and $y_1$ by $y_2$ for $V_3$. With no knowledge about the behavior of the other voters $V_2$ will, therefore, vote for $x_2$ and $V_3$ for $y_2$. $V_1$ knows this situation and consequently casts his votes for $x_2$ and $y_2$, since by doing so his most preferred outcome $(x_2, y_2)$ will get a majority.

But now voters $V_2$ and $V_3$ prefer $(x_1, y_1)$ to $(x_2, y_2)$ because of the higher payoffs $8>6$. And they can obtain this outcome by making a logrolling agreement. According to this agreement $V_2$ votes for $x_1$ as against $x_2$, which is against his immediate interest, to get in exchange $V_3$'s vote for $y_1$ as against $y_2$, which is contrary to $V_3$'s immediate advantage. This exchange of votes is, however, favorable to both, since $y_1$ strongly dominates $y_2$ for $V_2$, and $x_1$ strongly dominates $x_2$ for $V_3$. Consequently, a typical logrolling situation is present.

It is now quite obvious that in the case given above, logrolling does not imply the paradox of voting. For $(x_2, y_1)$ as well as $(x_1, y_2)$ are preferred to $(x_1, y_1)$ by only one voter. $(x_1, y_1)$ is a stable outcome. Note, moreover, that $(x_2, y_2)P_1(x_2, y_1)$, $(x_2, y_2)P_1(x_1, y_2)$ and $(x_1, y_1)P_1(x_2, y_1)$, $(x_1, y_1)P_1(x_1, y_2)$, where $P_1$ means "preferred to by $V_1$." As a consequence just the kind of complementarity is present, the absence of which had to be assumed to prove that logrolling situations imply the paradox of voting.[2]

I proceed to show that cyclical group preferences do not generally imply the presence of logrolling situations. First, it has to be mentioned that Koehler has not given a proof for cycles containing more than three alternatives. But it is obvious that cyclical group preference orderings with more than three alternatives $A_i$ can exist. In general, we have:

$$A_1PA_2PA_3P \cdots PA_{n-1}PA_nPA_1,$$

where $P$ means "preferred to by a majority of the group." Koehler's proof is only concerned with the case, in which $n=3$, and no proof has been given for $n>3$.

Secondly, consider Koehler's case with $n=3$:

$$A_1PA_2PA_3PA_1.$$

Even in this case the paradox of voting need not imply logrolling situations. For assume that $A_1$, $A_2$ and $A_3$ belong all to one issue $A$. For example, the $A_i$ may represent three different bridges, out of which one has to be selected by the group, since only one bridge can be built because of budgetary restrictions. In this case substantive logrolling is obviously not possible, since there are no two issues concerning which votes can be exchanged by group members.

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[1] David H. Koehler, "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," *American Political Science Review*, 69 (September, 1975), 954–960.

[2] Peter Bernholz, "Logrolling, Arrow-Paradox and Decision Rules: A Generalization," *Kyklos*, 22 (fasc. 1, 1974), 53, assumption A5.

The above considerations prove not only that Koehler has failed to show the logical equivalence of logrolling and the paradox of voting but also that such a proof is impossible. What Koehler has demonstrated—but only for three group members and for two issues—is that *under certain conditions* logrolling and the paradox of voting are equivalent. But his belief that he had given a general proof prevented him from trying to find these conditions. As mentioned above, one of these conditions is the absence of a certain kind of complementarity among the relevant issues.

# Some Political Implications of "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence:" A Comment*

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Although the theorem implied by Koehler's "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence," (and by the accompanying exchange between Bernholz and Koehler) has been proven elsewhere[1] in a more general form, the political consequences of the theorem have not yet been drawn out. Before specifying these consequences, it would be worthwhile to specify the theorem in what I believe is its most general form:

> *If unrestricted*[2] *vote trading is permitted with regard to a finite number of independent* (*i.e. separable*),[3] *M-sided issues*[4] *with any simple or special majority rule,*[5] *precisely those preferences which allow for an effective logroll,*[6] *lead to a voting cycle, or cyclic majority. Furthermore, if* (*in such situations*) *the individuals' preferences do not support a cycle then such a logrolling coalition is not efficacious.*[7]

This theorem has important substantive implications for political scientists,[8] to the extent that logrolling of non-complementary issues is a prevalent political phenomenon.

## Numerous Paths to Electoral Victory

The existence of cyclic majorities across a set of compound alternatives leads to a new critique of the description of political leaders as "seeking to win." This traditional characterization of leaders can be shown to be an underspecified definition of political rationality when there are cycles. In other words, the strategy alternatives available to politicians in cyclic situations contain numerous paths to victory. Thus, a definition of political rationality based on an assumption no more specific than "win-seeking" is not specific enough to yield definitive predictions. Furthermore, these alternative paths differ markedly from one another in terms of important political characteristics. To see how new degrees of freedom are available to politicians, and why there is need to further specify political motivation of leaders, consider the following example.

Assume an electorate of 70 million consisting of the following four disjoint subgroups of voters: 10 million blacks, 10 million farmers, 20 million labor union members, and 30 million "others," (See Table 1.) Imagine that each of the specific minority groups has one major "political concern" or issue about which its members feel passionately, i.e., they would rather win on that issue and lose on all the others than win on all the others and lose on that one issue. (Passionate feelings regarding any one issue therefore are defined relative to a larger set of issues.) In Table 1, the blacks passionately want the passage of civil rights legislation, the farmers want price supports, and the laborers want an end to "right-to-work laws." Further, assume that the members of each group are opposed to the preferred

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* I wish to acknowledge the careful and thoughtful criticisms I have received from a number of people including Duncan Black, Norman Frohlich, Leonard Lamm, Rick Piltz, Samuel Popkin, Thomas Schwartz, Gordon Tullock, and Oran R. Young. Norman Frohlich's kind loan of his favorite 70-million voter illustration (Figure 1) is greatly appreciated.

[1] Peter Bernholz, "Logrolling, Arrow-Paradox and Decision Rules: A Generalization," *Kyklos* 27 (fasc. 1, 1974) 49–62, as well as an independently derived formulation: Joe A. Oppenheimer, *Relating Coalitions of Minorities to the Voters' Paradox or Putting the Fly in the Democratic Pie,* paper given at the 1972 meeting of the Southwest Political Science Association, San Antonio, Texas, March 30–April 1, 1972. The first indication of the relationship probably was that contained in Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957), especially pp. 55–60 and 64–69.

[2] Works showing the consequences of restrictions on vote trading are John A. Ferejohn, *Sour Notes on the Theory of Vote Trading,* Social Science Working Paper, California Institute of Technology, 41 (June, 1974); Thomas Schwartz, "Collective Choice Separation of Issues, and Vote Trading," (Pittsburgh: Carnegie-Mellon, Feb., 1975) paper given at Foundations of Political Economy Conference, Austin, Texas, February 28–March 1, 1975; as well as Joseph B. Kadane, "On Division of the Question," *Public Choice,* 13 (Fall, 1972) 47–54.

[3] See Bernholz's "Comment," *American Political Science Review,* 69 (Sept., 1975).

[4] A generalized proof for $m$-sided issues ($M$ finite) is in Bernholz, "Logrolling."

[5] The generalization to any majority rule is in Oppenheimer.

[6] Of course, not all vote trades are logrolls. For a definition of logrolling, see Bernholz, "Logrolling," p. 53. A most informative discussion of the restrictive consequences of this definition relative to other possible vote trades can be found in Schwartz, "Collective Choice," pp. 18–20.

[7] With special majority rules, vote trading can be undertaken to block rather than to win a vote, hence the introduction of the notion of efficacious, cf. Oppenheimer.

[8] Except as noted all of what follows comes directly from Oppenheimer.

**Table 1. Illustration of Coalitions of Minorities: Cyclic Outcomes**

| Group | Size | Issue Positions | | |
|---|---|---|---|---|
| | | Price Supports for Farm Prod. | End Rt-to-Work Laws | Civil Rights |
| (F)—Farmers | 10 million | very pro[a] | anti | anti |
| (L)—Labor | 20 million | anti | very pro | anti |
| (B)—Blacks | 10 million | anti | anti | very pro |
| (O)—Others | 30 million | anti | anti | anti |

Headcount for each issue:

1. farm price supports: 10 million pro vs. 60 million anti
2. end rt. to work laws: 50 million anti vs. 20 million pro
3. civil rts: 10 million pro vs. 60 million anti

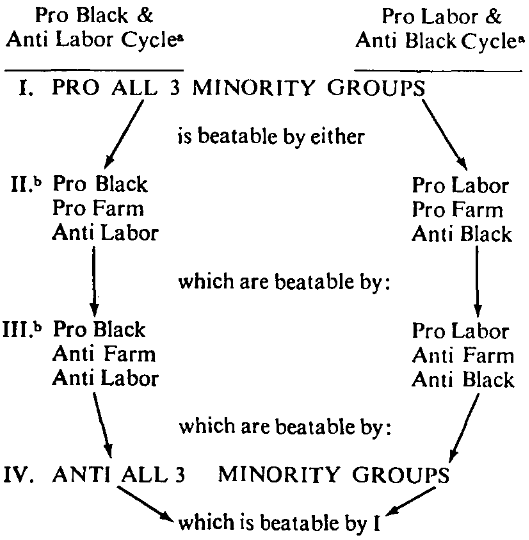
[a] "Very pro" means preferring to win on that issue and lose on the others than win on all the others and lose on that one issue, i.e., feeling passionately about that issue.

positions of the other groups. Thus, considered by itself, civil rights would fail (60 to 10), farm supports would fail (60 to 10), and pro-union legislation would fail (50 to 20).

But what would happen if coalitions formed to support multi-issued "platforms?" For example, consider the situation without coalitions. The minorities all lose, and the status quo becomes a "platform" which is opposed to all three of the identified minorities. Here the majorities on each of the issues win. (I shall designate this platform as MAJ.) On the other hand, if all of the minorities banded together and agreed to trade votes across issues to support their three passionately held issues, they could form a victorious coalition. That is, the coalition of minorities (designated as BLF) could beat the anti-minorities "platform": MAJ. But BLF could, in turn, be defeated by a platform which was in part pro, and in part anti, minorities. To illustrate, consider a platform supporting two minority positions and one majority position: pro-labor, pro-farmer, and anti-black (designated as LF). In a contest against BLF all the individuals who are anti-civil rights will support LF since LF differs from BLF only with regard to civil rights. Thus LF could garner the support of the 30 million others plus the farmers and labor. (See Table 2.)

But other winning proposals exist against BLF. A lemma by Sen which proves that any voting cycle can be reduced to cycles of three alternatives,[9] implies that numerous paths can be taken in order to secure victory, once one has arrived at the "pro-minority" position in the cycle. The opponents can, after all, kick out *any* one (or two) of the pro-minority planks and still win. To illustrate, consider running a pro-labor, pro-black, and anti-farmer platform (LB) against BLF. Now LB gains the support of all but the farmers. Or imagine that a pro-labor anti-black, and anti-farmer position (L) is proposed against BLF. It too, could win. In the first illustration, MAJ lost to BLF, which lost to LF. LF could lose to L which in turn could be beaten by MAJ. In this example, a four-stage cycle was constructed. The cycle chosen would allow labor to have its interests met in three out of the four stages of the cycle. Blacks, on the other hand, would be 'satisfied' only once during the four cyclic periods. Their roles could just as well have been reversed. That is, cycles have introduced alternative paths to victory. The strategies adopted could have been

**Table 2. Illustrative Alternative Logrolling Cycles**


Pro Black & Anti Labor Cycle[a]
Pro Labor & Anti Black Cycle[a]
I. PRO ALL 3 MINORITY GROUPS
is beatable by either
II.[b] Pro Black Pro Farm Anti Labor
Pro Labor Pro Farm Anti Black
which are beatable by:
III.[b] Pro Black Anti Farm Anti Labor
Pro Labor Anti Farm Anti Black
which are beatable by:
IV. ANTI ALL 3 MINORITY GROUPS
which is beatable by I

[a] These are merely 2 of a larger number of possible cycles.

[b] Either step II or III could be omitted from the cycle.

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[9] Amartya K. Sen, "A Possibility Theorem on Minority Decisions," *Econometrica,* 34 (1966), p. 492.

"pro-black and anti-labor" with labor the first minority group "kicked out" of the coalition and blacks the longest retained. Indeed, the larger the number of minorities put together in a coalition, the longer the maximum possible cycle and the greater the divergence possible between the favored and the nonfavored groups.[10] Clearly, the choice of whom to "support" and whom to kick out of any coalition of minorities is arbitrarily related to the objective of winning. But it is an essential element in the strategy choice of any politician motivated by the scent of victory. To predict his choices one must further specify the goals (or constraints) governing his behavior. Thus, such secondary factors as monetary rewards, size of electoral majority, and ideology are left to play a potentially important role in strategy selection for win-oriented politicians.

### Interpreting Electoral Reversals

To the extent that electoral choices reflect voters' paradoxes, one could have electoral reversals without changes in the opinions of the electorate. That is, if the assumptions are met, the changes in electoral outcome could reflect changes in coalition structure or changes of the options facing the voters rather than changes in the opinions of the electorate.[11] In other words, there are many potential majorities in any political constituency, and to explain political outcomes one must deal with the strategic relations between pairs of strategies and their place in an issue cycle.

### On the Value of a Democratic System

But are the coalitions which compete in a democracy likely to be logrolling coalitions? Perhaps, given the vulnerability of coalitions of minorities, other sorts of coalitions are constructed. But if coalitions of minorities do not form the basis for issue differences between parties, along what issues *do* the competitors differentiate themselves from one another? If issues are independent, coalitions could have two purposes.

First, coalitions could be needed to obtain a divergence from the MAJ platform which would win if each issue were designated in isolation.[12] Thus, coalitions can form for purposes of *explicit* logrolling.[13] But if they are to be effective (i.e. constitute a majority) when there are no impediments to vote trading, the package they support must contain minority positions on a set of issues and vote trades must take place between the minorities. That is, only a set of issues (call it a set of minority issues) which can support victorious coalitions of minorities will be chosen as the basis of such coalitions. Consider other issue sets. If the trades among minorities cannot be engineered to construct a majority, then we might define the set of issues as majority issues.[14] Assuming no costs of negotiation, effective coalition formation can not occur over these issues. A minority position on such an issue is sure to lose. If there is sufficient information to identify sets of issues as "majority" and "minority," then only those sets of issues about which one could construct coalitions of minorities will be involved in logrolling.

On the other hand, coalitions can form quite naturally out of the electoral process. That is, candidates take positions on sets of issues to attract voters, thereby implicitly constructing coalitions. This process of *implicit* logrolling can be analyzed as can that of explicit logrolling. The results are analogous. The parties will try to match on the majority issues and the party differential (in Downs's terms) will tend to consist of minority issues.

To the extent that parties are differentiated merely by minority issues, the outcome of the electoral process is *arbitrary*. More specifically, such a social choice would always be the arbitrary resultant of the order of the agenda—or of a procedure designed to break cycles arbitrarily. The stage in the cycle would determine the issues that would differentiate the parties in any particular period of time. Indeed, if only minority issues differentiated parties, then *with regard to issues that would be settled in the electoral arena, a democratic system would offer no advantages over a nondemocratic one*! That is, any benefits from democracy must occur as externalities of the electoral process. At least one of these externalities can be identified. Note that if only minority issues differentiated parties, *a democratic system leads to the settlement of majority issues by consensus*. These issues tend neither to be fought over by the parties nor raised in the political arenas. Such issues, then, are settled by being excluded from the process of competition, and the settlements are protected by the system of competition. Although these characteristics of democracy have usually not been of central concern to the democratic theorist,[15] it is clearly important to apply these findings to characterize democratic processes.

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[10] Perhaps it should be noted that "vote-maximizing," in contrast to "victory-seeking" politicians would tend to choose strategies which would lengthen the cycles.

[11] Of course, changes in the issues over time could also occur, as could the traditional explanation of changes in the preference orders of the members of the electorate.

[12] Kadane proves MAJ is the only winner without cycles.

[13] The distinction between explicit and implicit logrolling was first made by Gordon Tullock, "Some Problems of Majority Voting,'" *Journal of Political Economy*, 67 (December, 1959) 571–579.

[14] The usage parallels that of Downs in *Economic Theory*.

[15] An exception is Bachrach's and Baratz's concern with the "issues which are never raised," cf. Peter Bachrach and Morton Baratz, "The Two Faces of Power," *American Political Science Review*, 56 (December, 1962), 947–953.

For example, consider how we can use the notions of majority and minority issue sets to analyze why people vote. Majority issue sets have two characteristics which can be quite illuminating in an examination of why individuals vote. First, there must be a majority of individuals who agree as to which side of *each* issue is preferable if the issues constitute a majority set. Second, these individuals must agree on the importance of the issues: they must feel passionately about their preferences on these issues. Remember, the majority issues were those protected by the system, and not included in the party differential. Assume that a voter feels he is as likely to change an electoral outcome as to protect the issue-consensus. Then we could expect the average voter to go to the polls motivated not by the differences between the parties but by the support his vote gives to the consensus on the majority issues. This follows since majority issues would be made up of only "the most important" issues, as perceived by the majority of voters. Note that this sort of motivation might well be rationalized as a general interest in maintaining the system.

If we compare the relevant statistics regarding this in *The American Voter*,[16] we find preliminary corroborating evidence for the theory espoused here. In particular, 52 per cent of the voters who did not care at all about the outcome of the election, voted anyhow. But only 13 per cent of the voters who had a "low sense of citizen duty" voted. This would make sense if the "sense of citizen duty" question was actually tapping information regarding the citizen's interest in "maintaining the system." Downs discusses voting in order to "save democracy" in some detail, and we can now add a further detail in the argument he makes. If the assumptions of the argument are accurate, the utility of saving the system of democracy (for the majority of voters) would be greater than the utility of the party differential.

Furthermore, to the extent that the party differential is made up of minority issues, and to the extent that *all* issues in the group are minority issues, there will be no value to "saving the system." The further we move from consensus, the less difference will the existence of a democratic system make on the outcome of the policy choosing process.

## Implications for Further Research

Much of the theoretical work which has been done about the voters' paradox has been oriented toward identifying precisely those conditions which rule out the voters' paradox. It follows directly from the theorem that the empirical domain of assumptions which logically rule out the paradox are of limited applicability in situations involving independent issues. This should prove to be helpful in constructing models of particular political arenas. Thus, for example if one were to construct a model of the legislative process (where unrestricted logrolling of independent issues is prevalent), one would reject those assumption sets which rule out the paradox. On the other hand, politics in certain arenas and over particular issues might well be structured so no cycles occur. There one can adopt sets of assumptions (e.g., single peaked preferences) or observe certain structural features (e.g., gag rules)[17] which eliminate the paradox and allow for the construction of realistic models. With this in mind, one should be able to prescribe the domain for certain methodologies of analysis or models such as spatial models involving single peakedness.

Of course, the relaxation of the assumptions constituting the antecedent in the theorem will be of great interest. Thus, further work along these lines such as that by Schwartz and Ferejohn, where restrictions to vote trading are considered, will increase our ability to handle real-world political problems. For example, often when political situations are highly structured and manipulatible there are restrictions to vote trading. In these situations it should be possible and helpful to include the coalitional analysis in any model of manipulation.[18]

In summary, the identification of the voters' paradox with efficacious logrolling between independent issues establishes a number of serious consequences for empirical and normative theory of democracy. Further work in this area could be quite important for the development of a more rigorous, relevant, and realistic theory of democracy.

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[16] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1964), pp. 57, 58, and 59.

[17] John C. Blydenberg, "The Closed Rule and the Paradox of Voting," *The Journal of Politics,* 33 (February, 1971), 57–71.

[18] For an example of such a model, see Charles R. Plott and Michael E. Levine, *On Using the Agenda to Influence Group Decisions: Theory, Experiments and an Application,* Social Science Working Paper, California Institute of Technology, 66 (November, 1974).

# Vote Trading and the Voting Paradox: Rejoinder

DAVID H. KOEHLER

Bernholz's comment is useful in emphasizing limits on the generality of my proof of logical equivalence of vote trading and the voting paradox. The specific limit in the case of Bernholz's first example concerns an assumption which I have made and he has not. The situation is summed up by John A. Ferejohn, who states:

> The idea of . . . [separability] . . . is that the legislator's preference on any subset of bills can be unambiguously determined and does not depend on bills which have already passed. This assumption is found throughout the literature on voting trading in various guises. In particular, separability is necessarily satisfied if legislators have additive utility functions on the bills.[1]

In my paper, additivity is implicit in the analysis of utilities in Table 2, and it is assumed explicitly in the appendix where preference orderings over outcomes are derived from individual preferences on motions.

Bernholz's interesting example clearly violates additivity as he instead specifies complementarity among issues. Taking the utilities in his Matrix 1, $V_1$'s ordering of the social states is as follows:

$$(x_2, y_2)P_1(x_1, y_1)P_1(x_1, y_2)P_1(x_2, y_1).$$

From the first and third ranked states it appears that $x_2P_1x_1$. If additivity holds, it follows that $(x_2, y_1)P_1(x_1, y_1)$, but examination of his second and fourth ranks indicates that the preference relation is reversed. Hence, additivity is violated. Still, this does not detract from the example, and Bernholz's point is well taken. At the very least, the additivity assumption should have been made explicit in the text.

The second question, raised with respect to the number of alternatives considered, is answered by Sen's lemma which states:

> If among a set of alternatives there is no triple such that, given the set of individual preferences, the method of majority decision gives intransitive results between them, then the method will give consistent results for the entire set of alternatives.[2]

Thus, excluding the possibility of ties, a cycle containing more than three alternatives may be reduced to two or more cycles each containing no more than three alternatives.

Finally Bernholz's example of voting on three mutually exclusive alternatives demonstrates that it is possible to structure a situation such that vote trading cannot occur despite the existence of cyclical preferences. (The last example in my paper indicates that under the same conditions, an *incentive* to exchange votes still exists.) This also occurs if legislative rules, formal or informal, prohibit vote exchanges or communications among members. Likewise, it should be noted that the rules ordinarily preclude voting cycles: if the status quo wins over an amended bill, it (the status quo) is not further tested against the original bill. Thus the possible occurrence of a cycle is procedurally eliminated.[3]

A more interesting case is provided by Ferejohn who states a theorem:

> There exists a legislature with no majority winner and in which there is an equilibrium with respect to pairwise trading.[4]

The proof is based on a five-member body deciding two issues. In this case a social state is denoted (1, 0) which indicates an outcome of 1 on the first issue and 0 on the second. The *additive* individual preference orders are given as follows:

| | | Members | | |
|---|---|---|---|---|
| 1 | 2 | 3 | 4 | 5 |
| (1, 0) | (1, 0) | (0, 1) | (1, 1) | (1, 1) |
| (0, 0) | (0, 0) | (0, 0) | (1, 0) | (0, 1) |
| (1, 1) | (1, 1) | (1, 1) | (0, 1) | (1, 0) |
| (0, 1) | (0, 1) | (1, 0) | (0, 0) | (0, 0) |

The result of naive voting (voting on each issue alone) is (1, 1), but notice that members 1, 2 and 3 all prefer social state (0, 0) to (1, 1). Nevertheless, by limiting exchanges to pairwise trades an equilibrium prevails: no two members who may both prefer an alternative social state to (1, 1) can *by themselves* change the outcome.

At the same time that Ferejohn's proof specifies a limitation on the generality of the equivalence, it emphasizes the point I was trying to make, i.e., " . . . that the individual preferences required for vote trading are necessary and sufficient conditions for the voting pradox . . . ," and that the logic which underlies the two processes is the same. If the restriction to pairwise trades is lifted to allow all mutually beneficial exchanges (and it was noted in the text that " . . . three members can be three factions"), as indicated above outcome (0, 0) is preferred by three members to the naive outcome (1, 1), but also, (0, 0) is dominated by (1, 0) and (1, 0) in turn by (1, 1). If members become entangled in this unstable situation before a decision is taken, the process is vote trading, while if it occurs at the time of the vote, the result is a cyclical majority.

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[1] John A. Ferejohn, *Sour Notes on the Theory of Vote Trading*, Social Science Working Paper, California Institute of Technology, 41 (June, 1974), 8.

[2] Amartya K. Sen, "A Possibility Theorem on Minority Decisions," *Econometrica*, 34 (April, 1966), 492.

[3] William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1973), p. 112.

[4] Ferejohn, pp. 10–11.

Oppenheimer's comment notes that others have also established the equivalence between vote trading and the voting paradox. This is true as I have indicated in the paper (footnote 7). What occurred was a rare instance in political science of relatively simultaneous and independent discovery of an unrecognized relation between two important political constructs. The independence of the several pieces of work is indicated by differences in the proofs which have been offered. I would argue that the version presented here is of value as it demonstrates the relation directly in terms of the individual preference patterns which are commonly associated with trading and with the paradox. Deciding which is "best" among these proofs is partially a question of generality, but it also depends upon which provides the most tractable basis for further development, e.g., in the analysis of voting strategy. The latter question is still to be decided, and I can only leave that to the judgement of the reader.

Considering the substance of Oppenheimer's comments, I find little to argue with. The implications of the result for the analysis of both legislative and electoral strategy are profound. However, these applications do not specify identical behavior even though they share the same logic. As a result it is important to distinguish between vote trading and the coalition of minorities.

Downs conceptualized the coalition of minorities as an electoral strategy which might be successful for an opposition party seeking to unseat an incumbent following the "majority principle."[5] This strategy, however, has generally not been considered the same as making voting agreements in a legislature, and in that respect, the coalition of minorities is something of a misnomer. In an election, actors with differing preferences do vote alike, but not as a result of what would ordinarily be considered coalition formation. There is no agreement among voters to coordinate their behavior and, in fact, there need be no communication whatever among the minorities. This is important since it makes little sense to formulate an electoral strategy based on the assumption that large groups of voters are sufficiently disciplined to form and enforce agreements to act as one. The reason the coalition of minorities is a feasible electoral strategy is precisely because such collusion is not required. It is the candidates, rather than the voters, who behave strategically by manipulating the sets of issue positions which are within the scope of their control. In response, voters are not expected to "swap votes," or for that matter pursue any other type of strategic behavior. Indeed, for the coalition of minorities to be successful, the voters must honestly express their preferences with respect to the competing issue sets or platforms.

In legislative vote trading the situation is quite different, since the relatively small number of members allows for the possibility of securing agreements to vote systematically against individual preferences. In this case, the policy alternatives may remain fixed while individual votes are the subject of manipulation. In contrast to the coalition of minorities, vote trading actually approximates a coalition model; members form an agreement to adopt a single specific voting strategy as they confront known individual payoff functions.

Thus there are good reasons for failing to recognize the equivalence of vote trading and the coalition of minorities; they are applicable to different voting situations, and each prescribes behavior appropriate to one and not the other. The clue which points to equivalence emerges from the analysis of vote trading as it becomes evident that a member's willingness to switch votes depends upon joint consideration of two or more motions. This is made explicit in the salience condition which requires that a member's utility must increase in a single transaction as he exchanges a low salience winner for a high salience winner. In a sense, a platform is formed as a majority set of members agree to support each of its parts regardless of their individual preferences. The joint consideration by voters of a set of two or more issues, whether resulting from agreement among individual legislators or in response to an exogenously created platform, is the point on which vote trading and the coalition of minorities intersect.

Commenting on the political implications of the voters' paradox, Oppenheimer states that ". . . one could have electoral reversals without changes in the opinions of the electorate." In fact, it is unlikely that this would come as a shock to either spatial modelers or political candidates. If candidates' issue positions influence voting, this statement is true whether the paradox exists or not. The important point is that if the coalition of minorities is an efficacious strategy, there is no equilibrium: a candidate simply can find no platform which he prefers regardless of the platform chosen by his opponent. Beyond simply effecting electoral reversals, the existence of the paradox presents repeated opportunities for a candidate to *improve* his chance of electoral success by altering existing positions or by cultivating new minority issues.

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[5] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row Publishers, Inc., 1957), pp. 54–60.

The unanswered question concerns the existence of electoral disequilibrium in the real world. Oppenheimer indicates that parties will match positions on "majority issue sets" and differentiate themselves only on "minority issue sets." To the extent this actually occurs, his dismal projections seem unavoidable. On the other hand, matching on majority issue sets is not always possible. Beyond the fact that party tradition and credibility require some degree of platform consistency over time, the personal characteristics of the candidate—the image—are only subject to marginal adjustments. Thus if candidate evaluation is a dominant factor in the minds of the voters, and if personality matching is impossible, then a majority issue exists and the coalition of minorities can be ruled out as an efficacious strategy. This suggests that there may be good reason for a candidate to emphasize personality, and given the uncertainty surrounding minority issues, good reason for citizens as well to assign it substantial weight in deciding how to vote.

# COMMUNICATIONS

## Subcommittee Chairmanships in the House of Representatives

To the Editor:

Since the Legislative Reorganization Act of 1946, the role of the subcommittee structure in the decision processes of the House of Representatives has become of prime importance. Richard Fenno, Jr. has stated "When the 1946 Act reduced the number of standing committees from 48 to 19, it stimulated the growth and importance of subcommittees. This outcome has obscured the realities of committee-based influence. Analyses of the committee leadership which exclude the 123 (as of 1964) subcommittees can be but caricatures of the influence patterns in the House."[1] However, in spite of Fenno's indictment, analysis of the subcommittee structure of the House is still in embryo.

It is known that the primary foci of power in the subcommittee structure are the subcommittee chairmen.[2] Thus the assignment of subcommittee chairmanships is a key to the distribution of power in the House, and identification of possible norms in the assignment process critical to an understanding of the distribution of influence within the House.

Two such possible norms are examined here—full committee seniority and subcommittee seniority. Under the former mode, subcommittee chairmen would be those members of the respective subcommittee with the greatest period of continuous service on the parent committee, while under the latter, subcommittee chairmen would be those claiming the greatest period of continuous service on that particular subcommittee. Following the lead of Polsby, Gallaher and Rundquist,[3] the scope and usefulness of these definitions may be increased by introducing the concept of "compensation": A member is said to have received "compensation" if, when due a subcommittee chairmanship and denied, that member either receives or holds another position of influence, e.g., chairmanship of the full committee, chairmanship of another subcommittee, chairmanship of a different full committee, or chairmanship of a subcommittee on a different full committee. (The latter two types of compensation were extremely rare, occurring only twice in the cases studied.) Thus, in examining the assignment of subcommittee chairmen, in each instance in which the chairmanship of a subcommittee changes hands, we wish to determine (1) if the new subcommittee chairman is that member of the subcommittee with the greatest full committee seniority, or if not, whether all other members of that subcommittee with greater full committee seniority are given "compensation"; and (2) if the new subcommittee chairman is the member of the subcommittee with the greatest subcommittee seniority, or if not, whether all the other members of that subcommittee with greater subcommittee seniority are given "compensation."

These questions were employed in analyzing all changes in subcommittee chairmanship in the 80th-91st and part of the 92nd Congresses. (Except for short-lived ad hoc or special subcommittees—usually extant only one or two Congresses—or cases in which the entire subcommittee was reconstituted. The well-established special and select subcommittees of the Education and Labor Committee were included, however.) The data in Table 1 show the extent to which the above questions were answered in the affirmative.

As seen from the resulting data, of the 303 changes in subcommittee chairmanship, 300 or 99 per cent can be explained by the adoption of either the full or subcommittee seniority norm by each committee. The data also clearly indicate the norms which we may identify with each committee:

Full Committee Seniority

- Agriculture
- Armed Services
- Banking and Currency
- Education and Labor
- Foreign Affairs
- Government Operations
- Interior
- Interstate and Foreign Commerce
- Judiciary
- Merchant Marine and Fisheries
- Post Office and Civil Service
- Public Works
- Science and Astronautics
- Veterans Affairs

Subcommittee Seniority

- Appropriations
- District of Columbia
- House Administration

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[1] Richard Fenno, Jr., "The Internal Distribution of Influence: The House" in Raymond Wolfinger, ed., *Readings on Congress* (Englewood Cliffs, New Jersey: Prentice-Hall, 1971), p. 203.

[2] See George Goodwin, "Subcommittees: The Miniature Legislatures of Congress," *American Political Science Review*, 56 (September 1962), 596–604; Burton L. French, "Subcommittees of Congress," *American Political Science Review*, 9 (February, 1915), 68–92; Charles Jones, "The Role of the Congressional Subcommittee," *Midwest Journal of Political Science* (November, 1962), 327–441.

[3] Nelson W. Polsby, Miriam Gallaher, and Barry S. Rundquist, "The Growth of the Seniority System in the House of Representatives," *American Political Science Review*, 63 (September, 1969), 787–807.

**Table 1. Changes in Subcommittee Chairmanship**[a]

| Committee | Chngs. | S | S* | % | SCS | SCS* | % |
|---|---|---|---|---|---|---|---|
| Agriculture + | 29 | 15 | 13 | 96.5[b] | 17 | 6 | 82.1 |
| Appropriations + | 38 | 25 | 10 | 92.0 | 29 | 9 | 100[b] |
| Armed Services + | 12 | 12 | 0 | 100[b] | 4 | 0 | 33.3 |
| Banking and Currency + | 8 | 5 | 3 | 100[b] | 5 | 2 | 87.5 |
| Dist. of Columbia + | 31 | 15 | 9 | 77.4 | 19 | 12 | 100[b] |
| Education & Labor + | 9 | 6 | 3 | 100[b] | 2 | 1 | 33.3 |
| Foreign Affairs + | 27 | 11 | 16 | 100[b] | 10 | 4 | 51.8 |
| Gov't. Operations + | 14 | 9 | 5 | 100[b] | 8 | 0 | 57.1 |
| House Administration + | 15 | 13 | 1 | 93.3 | 15 | 0 | 100[b] |
| Interior | 26 | 3 | 23 | 100[b] | 5 | 19 | 92.6 |
| Commerce | 8 | 8 | 0 | 100[b] | 1 | 0 | 12.5 |
| Judiciary | 20 | 17 | 2 | 95.0[b] | 13 | 1 | 70.0 |
| Merchant Marine & Fisheries | 17 | 11 | 5 | 94.1[b] | 11 | 0 | 64.7 |
| Post Office & Civil Service | 13 | 11 | 2 | 100[b] | 12 | 0 | 92.3 |
| Public Works | 17 | 9 | 8 | 100[b] | 8 | 6 | 82.3 |
| Science and Astronautics | 3 | 3 | 0 | 100[b] | 0 | 0 | 0 |
| Veterans Affairs | 16 | 12 | 4 | 100[b] | 13 | 2 | 93.7 |

[a] *Congressional Index* (New York: Commerce Clearing House 1947–71). Data taken from 80th–91st Congresses except for Committees with +, wherein data taken from 80th–92nd Congresses.
[b] Peak correlation in method of determining subcommittee chairmen.

Chngs.: Changes in subcommittee chairmanship
S: Chairmanship Determinable by Full Committee Seniority
S*: Chairmanship Determinable by Full Committee Seniority with compensation
SCS: Chairmanship Determinable by Subcommittee Seniority
SCS*: Chairmanship Determinable by Subcommittee Seniority with compensation

Thus we see that the dominant mode of power distribution (99 per cent) in the subcommittee structure is full committee seniority, with the occasional variation of subcommittee seniority; we must therefore modify the common view of the role of the seniority system in governing the internal distribution of power in the House, that:

> The only important area in which seniority seems to play a role of overwhelming significance is in the matter of succession to the chairmanship of committees,[4]

by making the following addition:

On all but three of the committees of the House maintaining subcommittee structures, full committee seniority is the overwhelmingly dominant factor in the succession to the chairmanship of subcommittees, and hence, in the distribution of power within the committees themselves. On the remaining three committees, the employment of a subcommittee seniority norm decentralizes power to a limited extent, but still leaves a degree of seniority as the primary criterion for power within the committee, and consequently, within the House.

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[4] *Ibid.*, p. 787.

## The Authentic Rousseau

To the Editor:

I would like to respond briefly to Allan Bloom's diatribe against my book, *The Politics of Authenticity*, in your latest issue (September, 1974, pp. 1297–99). In a way it's nice to see that, even in the dismal 1970s, the fervid passions of the late 'sixties continue to seethe. Alas, as those who remember the 'sixties will recall, it's not easy to argue with a pot boiling over. My first impulse was not to bother; but I guess I'm not quite Olympian enough simply to shrug or laugh it off.

Bloom ascribes to me a belief that "any constraint of inclination is tyranny," that "no harm can come of any indulgence whatsoever," that morally "anything goes." I am supposed to assert with particular vehemence that "anything goes" in sexual life: that, says Bloom, is my "erotic faith." I can see how this image might help my book's sales—at least it might have if it had appeared four years ago, when the book came out—but it springs from Bloom's imagination, not mine. I do emphasize Montesquieu's and Rousseau's attacks on sexual repression, and I unfold their arguments that erotic feelings and relationships are crucial to the development of the self. But I am just as emphatic in arguing that, especially for Rousseau, sexuality can be "authentic" only when it generates deep and enduring human bonds: love, fidelity, commitment. The center of my book is an explication of romantic love as "the primary community," at once an heuristic model and a vital precondition for a wider, political community. "Thus, in making him a lover, I'll make him a good man" (*Emile*); "From now on, only love can guarantee your virtue" (*La Nouvelle Héloïse*): this is the only "erotic faith" in my book. I can see why some readers might find this romanticism naive or sentimental; I can see, too, why other readers might find my whole idea of authenticity ambiguous, elusive, deeply problematical; but for love or money I can't see how Bloom can see it as a general license to swing. I share Bloom's desire to distinguish serious scholarship from propaganda; but if his reading of my book is a fair sample of his scholarship, I feel for, and fear for, the other writers whose books he reads.

As for Bloom's nomination of me as a "hippy Savonarola": although it's a striking image—easily the best thing in the piece, if you ask me—I doubt that I'm fit to be keeper of this particular flame. Indeed, the virulence of Bloom's rage against my book suggests that he himself is carrying an awful lot of the Savonarolan fire within. Maybe that's what makes his review so inflammatory.

Bloom is especially incensed by my portrait of Rousseau as a divided self. "Like a magician in a circus, [Berman] cuts Rousseau in half; what once was an organic whole becomes two distinct personages . . . ." I am said to treat his works "as though they were written by Dr. Jekyll and Mr. Hyde taking turns sentence by sentence." Actually, this caricature is not bad at all. In fact, these lurid images—of magic, mangling, and mutilation, of the haunting of a "good" self by a monstrous antiself—are precisely the images that, for over two hundred years, have been most persistently used to describe Rousseau himself. Both his nearest and dearest friends (e.g., Diderot) and his worst enemies tended to portray him this way; moreover, as a random glance into his autobiographical writings will show, he tended to see himself this way. These images define the brilliantly imaginative but tragically tormented soul that is "my" Rousseau. I have explored and explained him, loved him and hated him for years, and I will stand by him to the end. However, in taking my stand, I have no desire to bring the study and interpretation of Rousseau to an end. I realize there are other Rousseaus—not only, as Bloom suggests, from sentence to sentence, but often within a single sentence. Bloom's Rousseau, for instance, is smooth and static as a Grecian urn, a walking Platonic Idea. And why not? There are more than enough Rousseaus to go around, plenty for all of us to cherish and cultivate. Bloom sees mine as a rank and poisonous weed that must be rooted out, while his prize "organic wholes" look like prefabricated, plastic flowers to me. Still, peaceful coexistence should be possible. In the field we share, the grand old common ground of Political Theory, there is surely enough room to let a hundred flowers bloom.

MARSHALL BERMAN
*The City University of New York*

TO THE EDITOR:

Mr. Berman's letter is a fair sample of the style and substance of his book. It requires almost no response, for it addresses itself to none of the precise criticisms I made concerning inaccuracies in his reading of both Montesquieu and Rousseau. Moreover, he admits his belief that writers in general and Rousseau in particular are pretty much what one wants to make of them—a belief I ascribed to him and one which vitiates serious interpretation. He treated his classic authors as imperfect precursors of his views which were the product of "the fervid passions of the late 'sixties," now apparently exhausted.

The only point his letter makes is that Mr. Berman is not a swinger. A close examination of my review will reveal that I made no such allegation. Nor did I say his view of Rousseau is "a rank and poisonous weed that must be rooted out." I suggested only that he wrote a poor book.

Mr. Berman says that I say that according to him "no harm can come of any indulgence whatsoever." My sentence continued " . . . so long as it comes from an authentic self." And I will stick by that formulation, which also implied that "authentic self" is an empty phrase because nowhere does Mr. Berman provide criteria for determining what an authentic self is. He does indeed praise Montesquieu and Rousseau for their "attacks on sexual repression," but he also attacks them ferociously for any restraints they favor—identifying restraint and sublimation with repression. His disclaimers would be more impressive if at any point in his book he told us about limits. Sexuality, he tells us, must "generate deep and enduring human bonds." That is also Rousseau's belief, and he presents the most detailed account of the conditions of "deep and enduring human bonds" ever given. The sexual limits he proposes are part of that account, and these limits Mr. Berman understands to be tyrannical and an expression of Rousseau's divided self. He never for an instant takes seriously the notion—not unique to the "tormented" Rousseau but accepted by most serious men of the past—that there is a tension between free sexual expression and enduring bonds. I did not deny that Mr. Berman wants such bonds. But I do insist that his book teaches that the removal of all limits, constraints and prohibitions is the way to such bonds. This is an undemonstrated assertion.

ALLAN BLOOM

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### Pessimism and Politics

TO THE EDITOR:

The essay by Glenn Tinder, "Transcending Tragedy," appearing in the June, 1974 issue of the *APSR* is extraordinary for its originality, synthesis of a vast amount of thought in a brief compass, and contemporary relevance. The author has made a powerful case for "civility" as a stance to be taken toward contemporary politics and life.

I could not agree more with Tinder's penetrating analysis of the ideal of community, emphasis on the "limits and unreliability of action" (p. 551), concern with the pitfalls of unreserved commitment to any ideology or group and his criticism of "the pride involved in thinking man can control history" (p. 553). His criticisms of Marxism are most apt. He provides a useful warning against "fanaticism in action" and "absolutism in beliefs" (p. 560).

A recurring theme of the article is that the events of this century have been "disappointing" and "tragic." Tinder has used the term "tragic" in its everyday sense to refer to the misfortunes and sufferings, individual and collective, which afflict men, rather than in its more technical meaning in drama, although there is certainly an overlap between the two meanings. In the plays of Greek tragedians such as Aeschylus, Sophocles, and Euripides there are forces confronting each other which each have legitimacy. A tragic hero denies balance and goes beyond a limit, and death and punishment follow. Destiny is fulfilled in spite of the revolt of the tragic figure.

There is a similarity between the fatalistic themes of great tragic dramas and their concern with *hubris* and Tinder's awareness of the extreme difficulty of controlling history and the pitfalls of self-righteousness. However, following Tinder, I will employ the term "tragedy" in its common usage in this commentary.

The author maintains that "we are not forced to become doctrinaire pessimists" (p. 558) and that "civility is not dogmatic pessimism" (p. 559). While well aware of the presence of injustice in the world, he seeks to avoid a pessimistic conclusion by asserting that "what we are morally bound to will and what we can reasonably expect are not always the same" (p. 559). Moreover, he seeks to uphold the values of "the suffering inherent in civility" (p. 557) and "the immeasurable dignity of the human" (p. 560). Although sharing broad areas of agreement with the author, I find myself bound to raise doubts from a utilitarian ethical perspective and a pessimistic ontology.

Tinder upholds the ethical value displayed in the lives of such individuals as Socrates, Jesus, Lincoln and Solzhenitsyn. No doubt, such values as "recognition of others" (p. 551), being "unreservedly attentive" (p. 554), and "solitary communality" (p. 555) can be seen in studying the lives of these men. Perhaps what they achieved is more admirable than "happiness," which Tinder sees as a "superficial" (p. 557) value.

However, utilitarians have made the point that numbers count. For every individual who reaches the moral heights extolled in the article, there are thousands or millions whose lives are mediocre or worse from a moral viewpoint. Many are broken by suffering rather than achieving dignity through it. It is doubtful that the tragedy of human existence can be redeemed by emphasizing the moral stature of a small minority of individuals. The suffering in the world, today as well as in the past, almost certainly outweighs the happiness in it, and thus from a utilitarian standpoint it becomes morally hard to defend the world's existence.[1]

If we observe that the human situation is gradually improving, or if some person or group discovers a way to change the world so as to make it a good place (i.e., one in which everyone has a decent life or at least a good chance for this), *then* let us discard pessimism. Until such time, the argument for pessimism is overwhelming. By pessimism I mean the philosophical position which asserts that the nonexistence of the world would be better than its existence and that this will continue to be so. One need not accept all of the more extreme or dogmatic statements of Arthur Schopenhauer to be a pessimist. A simple consideration of the enormous physical pain and suffering which afflicts humans and animals points toward the pessimistic conclusion. However, when one adds psychological suffering and moral evils (e.g. murder, rape, offensive wars, and war atrocities if not all war, dishonesty, ill will), it ought to be clear to a morally sensitive individual that the unfortunate aspects of life, particularly human life, outweigh its positive features.[2]

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[1] See St. Augustine's detailed description of "the host of cruel ills" with which this life is filled, "if life it is to be called," *The City of God* (New York: Random House, 1950), Bk. XXII, chap. 22. Rousseau observed that man "is born to suffer" and proclaimed: "Ever more sorrow than joy—this is the lot of all of us," *Émile* (New York: Dutton, 1972), pp. 15, 44.

[2] Even Plato, who believed in the possibility of eternal salvation and cannot be considered a philosophical pessimist, observed that "the good things in human life are far fewer than the evil . . ." *The Republic of Plato,* trans. F. M. Cornford (New York: Oxford University Press, 1970), p. 71.

Tinder has emphasized that which "we are morally bound to will." Surely the great amount of good will in the world should not be overlooked. Yet the existing good will and the hypothetical possibility of closing the gap between what we are morally bound to will and what we can reasonably expect are not sufficient to outweigh sufferings and moral evils which are real and present and show no signs of ceasing.

Those of a "negative utilitarian" persuasion and some other philosophers would join Tinder in his criticism of making positive happiness the primary value.[3] Rousseau, for example, said that man's "happiness in this world is but a negative state; it must be reckoned by the fewness of his ills."[4]

Is there then something beyond the great moral stature of the few or earthly happiness? Tinder refers to "salvation" in quoting Solzhenitsyn (p. 557) and to a number of religiously oriented philosophers, such as Berdyaev and Jaspers. In addition, there is appeal to "the ancient sense that there is a Logos, or meaning, deeply embedded in reality." Moreover, it is said that hope "is not condemned" (p. 558).

Now, one is more in tune with reality, in my view, in accepting that there is no salvation, either on this earth or in some other-worldly existence, either for the individual or for mankind. The yearning for salvation is understandable, but probably based on wishful thinking. A hopeful outlook is often very helpful to individuals as they go about their day-to-day lives. However, in view of the unfortunate realities of hunger, starvation, disease, the infirmities of old age, illiteracy, crime and war, the situation of hundreds of millions of humans in the world today is hopeless, and it would have been better if many had not been born.[5] Nor is there reason to believe that the situation of the human race is better than two hundred or two thousand years ago.[6] There have been great advances in science and medicine and a decline in certain cruel institutions (e.g. slavery), but the twentieth century has seen totalitarianism, the Nazi concentration camps, and unprecedented numbers killed or injured in war, among other horrors. Tinder asserts that to "hold that history *must* be disastrous and that general, long range progress *cannot* occur expresses "despairing pride" (p. 558). Possibly so, yet the pessimistic view may also be a realistic assessment of what has happened and will probably continue to happen.

The question arises as to what is the alternative. One hope, though surely a distant one, is that large numbers of both ordinary citizens and government leaders will begin to take seriously the ideal of civility which Tinder has delineated for us so well. Moreover, efforts to prevent war and struggles against hunger, disease and ignorance are admirable. By now I have begun to sound like a reformer, who believes it is possible, after all, to build a better world. But as long as men are on earth it is not desirable to discourage their efforts to realize whatever accomplishments *may* be possible.

In searching further, one finds relevance in the thought of Machiavelli, who observed that "one never tries to avoid one difficulty without running into another" and maintained that "prudence consists in being able to know the nature of the difficulties, and taking the least harmful as good."[7] I will suggest applications of this principle to just a few problems. Government programs to control growth in population are badly needed in many parts of the world, and compulsory sterilization, while not a pleasant alternative, is needed in some areas. Those dying of painful diseases ought to be allowed to take their lives (it is already permitted in hospitals in Switzerland), and we ought to struggle to remove the taboo against this alternative. Mercy killing of severely retarded persons and very ill elderly persons, when they are suffering a great deal, could alleviate much misery. Mere life, unless there is some capacity for satisfaction or productivity or expressed desire to live, has no value, and it may even be a disvalue.

The chances of achieving real progress on a worldwide scale are very minimal. Whether or not it is better for men to realize this is a difficult question, but probably it is better that they face the hard truth. Many individuals transcend tragedy for periods of time in their own lives and in their human relationships. Tinder and others have made noble efforts, but it is unlikely that the basically tragic nature of human existence will ever be transcended. Even philosophical pessimists, while attempting to understand history, are also struggling within it and subject to its tragic aspects.

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[3] Negative utilitarianism explicitly emphasizes giving priority to the relief of suffering over promoting happiness. See H. B. Acton and J. W. N. Watkins, "Negative Utilitarianism," *The Aristotelian Society*, supplementary vol. 37 (1963), 83–114.

[4] Rousseau, *Émile*, p. 44. For another expression of this view see Arthur Schopenhauer, "On the Sufferings of the World," in *The Pessimist's Handbook*, ed. by Hazel E. Barnes (Lincoln: University of Nebraska Press, 1964), p. 131. I believe that positive joy is possible but uncommon. The statements of Rousseau and Schopenhauer have great relevance as broad generalizations. Pain and suffering are not only more prevalent than pleasure and happiness; more importantly, they are more intense.

[5] Schopenhauer was inclined toward extremes, but many of his arguments for pessimism are worth considering. He claimed: "If you try to imagine, as nearly as you can, what an amount of misery, pain and suffering of every kind the sun shines upon in its course, you will admit that it would be much better if, on the earth as little as on the moon, the sun were able to call forth the phenomena of life; and if, here as there, the surface were still in a crystalline state." Schopenhauer, "On the Sufferings of the World," in Barnes, p. 129.

[6] Machiavelli's idea that "as a whole, the world remains very much in the same condition, and the good in it always balances the evil" is a plausible expression of the view that overall progress in the human condition is extremely difficult and maybe impossible. Niccolò Machiavelli, *The Discourses*, introduction to Bk. II.

[7] Machiavelli, *The Prince*, chap. XXI.

MICHAEL C. STRATFORD
*Central Michigan University*

TO THE EDITOR:

I am surprised (as well as interested) by the nature of Michael Stratford's criticism. It does not express the notion that at bottom civility is only acquiescence in the evils of established society—a criticism I had anticipated. Nor does it express a realization that civility as I define it does not imply respect for established institutions—an objection I thought might possibly be made by conservatives. His criticism rather is based on a deeply pessimistic utilitarianism, and the central point of it is apparently contained in the statement that, "It is doubtful that the tragedy of human existence can be redeemed by emphasizing the moral stature of a small minority of individuals."

Despite the implication here that the decisive flaw in civility is its presumed confinement to "a small minority of individuals," and despite Stratford's assertion elsewhere that "numbers count," I do not believe that the primary issue is that of numbers. For one thing, I see no good reason for thinking that civility is beyond the capacity of the majority; in my article I argue against the idea that it is an elitist virtue (p. 560). Aside from this, however, even if civility were universal, it is hard to see what value it would have in a world envisioned according to Stratford's pessimistic utilitarianism. I do not understand Stratford's grounds for conceding that civility might be more admirable than happiness. How could that be, for a utilitarian? This points to what the primary issue, perhaps, really is: not numbers, and not even pessimism (so far as historical prospects are concerned, we are in agreement there), but rather whether Stratford's utilitarianism provides a basis for anything but an extreme and devastating incivility.

The question, as I see it, is whether utilitarianism provides philosophical grounds on which it is possible to affirm the absolute value of every individual. It would be difficult to show that any such value is implied by, or can even be accommodated within, a philosophy that equates happiness and the good. If the absolute value of the individual is not affirmed, however, then it seems to me that there is no basis for civility—not only for the particular combination of attentiveness, detachment, and political responsibility that I called "civility" in my essay, but for civility of any kind, including that which Stratford evinces in his concern for reducing the sufferings of mankind. If each individual does not have absolute value, why (where my own welfare is not involved) should I care about the welfare of great numbers of individuals? On what basis does Stratford assert that "efforts to prevent war and struggles against hunger, disease, and ignorance are admirable"? If those efforts and struggles do not enhance my happiness, why should I support them?

If it could be made out on utilitarian grounds that each individual is of absolute value, however, then I think Stratford would encounter another serious difficulty in defending civility. In this case his pessimism would become unqualified and morally crushing, for if even one individual in all of history had gone through an unhappy life, the justification of the world would, on utilitarian grounds, be thrown into question. The pessimism that Stratford expresses in his comments is somewhat limited and tentative; it does not bar all hope and thus leaves room for struggling against war, disease, and other evils. But given a pessimism that asserts conclusively that it would be better if the world and mankind did not exist, surely nothing but a nihilistic morality could follow.

Stratford's comments are rich and thoughtful, and I might have drawn the line between us at some other point. For example, is it consistent for him to commend my emphasis on the limits of man's understanding and control, yet to be so confident of our powers of rational judgment as to call for policies (of mercy killing, and so forth) that would allow us to determine conclusively whether or not the lives of others have value?

To keep the present exchange within bounds, however, it is perhaps permissible to oversimplify. It is for this reason that I limit myself to asking how, in view of the principles he invokes in commenting on my article, Stratford can defend not only civility but any kind of concern for the public good except so far as one's own personal happiness is significantly and clearly involved? Of course one might find happiness for oneself in the happiness of others. But in that case, what stands between someone of Stratford's persuasion and the ethics of Dostoevsky's Grand Inquisitor? On the other hand, one may be less concerned than was the Grand Inquisitor with the happiness of all. In that case, what stands between a pessimistic utilitarian and—calling on another example from Dostoevsky—the ethics of "the possessed"?

GLENN TINDER
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# EDITORIAL COMMENT

**Off Our Backs, Change.** If anybody doubts there is something amiss with journals of political science, they need look no farther than *Change* magazine, a publication that covers issues in higher education. In discharging this mission, *Change* has undertaken to sponsor reviews of journals. Books, after all, get reviewed, why not journals?

One reason, we suspect, is that it is awfully hard for a single reviewer or team of reviewers to appraise with any competence the substantive contribution of a large and disparate collection of articles. In volume 68, for example, of our humble *Review*, we published 56 articles, 7 review essays, and one presidential address, not to mention a clutch of comments, rejoinders, communications and several hundred book reviews. That's a whole lot of reading, and only for one journal of political science, for one year.

Never mind. "Journals in political science," say our critics " . . . are badly written, jargonistic, and overly concerned with professional issues. . . . [P]olitical science as a profession is an obstacle to the study—let alone the knowledge—of political things."[1]

Decrying the tendency toward high-cost research technology, the critics proceed to offer nothing in the way of evidence to back up this sweeping indictment save—heaven help us—a table, classifying the contents, by subfield, of six journals of the profession. From this table, they deduce such dubious disclosures as "public law and international politics are clearly step-children in the discipline," and "we do not find it accidental that second-class status is accorded the two fields traditionally most involved in questions of policy." This pejorative language is used to describe the finding that by the lights of the critics disproportionately few articles in the indicated subfields appear in the six journals surveyed. No mention is made of the curious fact that both subfields are blessed with several unusually good and prestigious journals of their own. Why don't political scientists care more about policy? Not only are we too "scientific," say the critics, but we are too specialized, unwilling to risk prediction, too narrow, too slow on the draw.

And that about wraps up the political science journals, folks. Without actually looking inside any article, comment, rejoinder, communication, or review, it is possible to tell all these things about the output of several hundred scholars.

We were about to throw in the towel and suggest a bonfire at the Banta Company when a kind friend sent us a couple of other articles from *Change*, namely the reviews of the economics and sociology journals.[2] Amazing! Apparently, exactly the same things are wrong with our neighbors as afflict us: "This material is narrowly conceived and written in technical detail that makes it forbidding to outsiders. . . . " "For relevance to contemporary issues, you'd best do your own thinking."[3]

"Let not the unwary long be deceived about the direct applicability to life and living of scholarly ruminations."[4] "If once in a while something interesting and accessible to a public wider than the specialists breaks the editorial barrier, it will be luck rather than design. Don't count on it. . . . "[5]

This marvelous uniformity of complaints across disciplines suggests any number of amusing possibilities. Of course one possibility is that all several thousand of us in economics, sociology and political science have somehow gotten off on the wrong foot and what we needed at an early age was more instruction from the likes of *Change* magazine. Another possibility is that there's something about a sampling of professional journals that tends to reflect the actual accomplishments of scholarly work and this work is, on average, just about average, and therefore couldn't possibly aspire to stimulate the jaded palate of a true connoisseur of scholarship.

Another possibility is that there's a *Change* line about these matters, and any journal review from those precincts will reflect a preference for musings of global import and a corresponding contempt for the grubby efforts of scholars. We have no idea what the correct explanation is. We are certain that had the critics at least of the political science journals stooped to do a little actual reading in connection with their task, they might not have filled their nostrils with heady nectar, but they might have learned quite a lot about such things as issue voting, social origins of liberal democracy, recent trends in trust in government, the consequences of party reform and other rules on the political nomination process, and, perhaps especially useful, the idea of civility.[6]

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[1] Wilson C. McWilliams and Alan M. Cohen, "The Private World of Political Science Journals," *Change*, September 1974, p. 53.

[2] Robert Lekachman, "The Economics Journals," *Change*, September 1972, pp. 59–61. Randall Collins, "Surveying the Sociology Journals," *Ibid.*, Winter 1972–73, pp. 70–74.

[3] Collins, p. 74.

[4] Lekachman, p. 59.

[5] *Ibid.*, p. 61.

[6] See Glenn Tinder, "Transcending Tragedy: The Idea of Civility," *American Political Science Review*, 68 (June 1974), pp. 547–560.

**Thanks, Folks.** For the first time, the memory of person runneth not to the contrary, hard evidence exists that political scientists believe in the *APSR*. How do we know this? In a sample survey of *APSA* members, over two-thirds (70 per cent) said that the *APSR* was "excellent" or "good."[1] Yet only 21 per cent said they read the greater part of the journal, and a mere five per cent claimed to swallow each issue whole.

As we have often said, selective reading of the *APSR* makes the most sense to us, given the enormous range of concerns that animate the various members of our discipline. But it is heartening, all the same, to see that members of the profession are willing to extend good wishes to those whose interests differ from their own in the form of an endorsement of a journal that conscientiously publishes some material they personally do not read.

### Articles Accepted For Future Publication

Joel D. Aberbach, University of Michigan and Bert A. Rockman, University of Pittsburgh, "Clashing Beliefs Within the Executive Branch: The Nixon Administration Bureaucracy"

Paul R. Abramson, Michigan State University, "Generational Change and the Decline of Party Identification"

Christopher H. Achen, Yale University, "Mass Political Attitudes and the Survey Response"

C. Arnold Anderson, University of Chicago, "Conceptual Framework for Political Socialization in Developing Societies"

Francisco Arcelus and Allan H. Meltzer, Carnegie-Mellon University, "The Effect of Aggregate Economic Variables on Congressional Elections"

John A. Armstrong, University of Wisconsin, "Mobilized and Proletarian Diasporas"

John M. Bacheller, Kirkland College, "Lobbyists and the Legislative Process: The Impact of Environmental Constraints"

Richard M. Bank, University of California, Santa Barbara and Steven R. McCarl, University of Denver, "Virtue, Obligation and Politics: Revisited"

Charles D. Cary, University of Iowa, "A Technique of Computer Content Analysis of Transliterated Russian Language Textual Materials: A Research Note"

Jonathan D. Casper, Stanford University, "The Supreme Court and National Policy Making"

Roger Cobb, Brown University, Jennie-Keith Ross, Swarthmore College, and Marc Howard Ross, Bryn Mawr College, "Agenda Building as a Comparative Political Process"

David Collier, Indiana University, and Richard E. Messick, Office of U.S. Senate, "Prerequisites Versus Diffusion: Testing Alternative Explanations of Social Security Adoption"

Richard K. Dagger, University of Minnesota, "What is Political Obligation?

Fred R. Dallmayr, Purdue University, "Beyond Dogma and Despair: Toward a Critical Theory of Politics"

John P. Diggins, University of California, Irvine, "Four Theories in Search of a Reality: James Burnham, Soviet Communism, and the Cold War"

George Edwards, Tulane University, "Presidential Influence in the House: Presidential Prestige as a Source of Presidential Power"

Norman I. Fainstein, Columbia University and Susan S. Fainstein, Rutgers University, "The Future of Community Control"

Robert C. Fried, University of California, Los Angeles, "Party and Policy in West German Cities"

Benjamin Ginsberg, Cornell University, "Elections and Public Policy"

R. Kenneth Godwin and W. Bruce Shepard, Oregon State University, "Political Processes and Public Expenditures: A Re-examination Based on Theories of Representative Government"

Fred I. Greenstein, Princeton University, "The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies"

Susan Blackall Hansen, University of Illinois, Urbana, "Participation, Political Structure, and Concurrence"

Russell Hardin, University of Pennsylvania, "Hollow Victory: The Minimum Winning Coalition"

Fred M. Hayward, University of Wisconsin, Madison, "A Reassessment of Conventional Wisdom About the Informed Public: National Political Information in Ghana"

Douglas A. Hibbs, Jr., Massachusetts Institute of Technology, "Industrial Conflict in Advanced Industrial Societies"

David K. Hildebrand, University of Pennsylvania, James D. Laing and Howard Rosenthal, Carnegie-Mellon University, "Prediction Analysis in Political Research"

Robert W. Jackman, Michigan State University, "Politicians in Uniform: Military Governments and Social Change in the Third World"

M. Kent Jennings and Gregory B. Markus, University of Michigan, "The Effect of Military Service on Political Attitudes: A Panel Study"

M. Kent Jennings, University of Michigan and

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[1] Now we will reveal our favorite reading matter: *P.S.* VII, Fall, 1974, pp. 382–384, an article by Thomas E. Mann titled "Report on a Survey of the Membership of the American Political Science Association."

Richard G. Niemi, University of Rochester, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations"

Sam Kernell, University of Minnesota, "Presidential Popularity and Negative Voting: An Alternative Explanation of the Mid-Term Congressional Decline of the President's Party"

James I. Lengle, University of California, Berkeley and Byron Shafer, Russell Sage Foundation, "Primary Rules, Political Power and Social Change"

Michael Margolis, University of Pittsburgh, "From Confusion to Confusion—Issues and the American Voter (1956–1972)

Richard D. McKelvey, Carnegie-Mellon University, and John H. Aldrich, Michigan State University, "A Method of Scaling, With Applications to the 1968 and 1972 Presidential Elections"

Richard D. McKelvey and Peter C. Ordeshook, Carnegie-Mellon University, "Symmetric Spatial Games Without Majority Rule Equilibria"

R. D. McKinlay and A. S. Cohan, University of Lancaster, "The Political, Military, and Economic Performance of Military and Non-military Regime Systems: A Cross-National Aggregate Study"

Arthur H. Miller, Warren E. Miller, Alden S. Raine and Thad A. Brown, University of Michigan, "A Majority Party in Disarray: Policy Polarization in the 1972 Election"

Helmut Norpoth, University of Cologne, "Explaining Party Cohesion in Congress: The Case of Shared Policy Attitudes"

Fritz Nova, Villanova University, "Political Innovation of the West German Federal Constitutional Court: The State of Discussion on Judicial Review"

Karen Orren, University of California, Los Angeles, "Standing to Sue: Interest Group Conflict in the Federal Courts"

Benjamin I. Page, University of Chicago, "The Theory of Political Ambiguity"

Thomas L. Pangle, Yale University, "The Political Psychology of Religion in Plato's *Laws*"

James L. Perry, University of California, Irvine and Charles H. Levine, Syracuse University, "An Interorganizational Analysis of Power, Conflict, and Settlements in Public Sector Collective Bargaining"

Douglas Rae, Yale University, "The Limits of Consensual Decision"

David Resnick, Cornell University, "Crude Communism and Revolution"

Neil R. Richardson, University of Texas, Austin, "Political Compliance and U. S. Trade Dominance"

Michael J. Robinson, The Catholic University of America, "Public Affairs Television and the Growth of Political Malaise: The Case of *The Selling of the Pentagon*"

Austin Sarat, Yale Law School and Joel B. Grossman, University of Wisconsin, Madison, "Courts and Conflict Resolution: Problems in the Mobilization of Adjudication"

Paul R. Schulman, University of Tennessee, "Non-Incremental Policy Making: Notes Toward an Alternative Paradigm"

Peter J. Steinberger, University of California, Riverside, "Hegel as a Social Scientist"

J. Weinberger, Michigan State University, "Hobbes's Doctrine of Method"

Lynn T. White, III, Princeton University, "Local Autonomy in China During the Cultural Revolution: The Theoretical Uses of an Atypical Case"

Fred H. Willhoite, Jr., Coe College, "Primates and Political Authority: A Biobehavioral Perspective"

# Crises and Sequences in Collective Theory Development*†

ROBERT T. HOLT AND JOHN E. TURNER
*University of Minnesota*

With the appearance of the seventh volume in the highly visible series "Studies in Political Development," the Social Science Research Council's Committee on Comparative Politics has reached a new stage in its study of the dynamics of political development. The chairman presents it as "both a report on work accomplished and an introduction to a new departure in comparative analysis."[1] Far more than the previous volumes, this book represents the intellectual achievements of the Committee. The earlier works were edited by Committee members—and sometimes they authored individual chapters—but about 80 per cent of the contributors were not affiliated with the Committee. By contrast, all of the authors of *Crises and Sequences* are Committee veterans, and they have had opportunity to interact regularly over a lengthy time span, to call upon experts from a variety of disciplines, and to plan their research on a long-term basis with a reasonable assurance of funding.

In a last Committee report, Lucian W. Pye, the present chairman and an author of *Crises and Sequences*, observed that the "most important criterion in evaluation of any research activity presumably is the quality of the published products; . . . "[2] Since *Crises and Sequences* is in part a "report on work accomplished," a review and evaluation of this volume is, by the Committee's own standard, an appraisal of its research activities insofar as they are covered in this work. In undertaking an evaluation, we must first consider the Committee's goals and the resources available to pursue them.

## The Committee: Goals and Resources

In 1954, when the Committee on Comparative Politics was formed, "comparative government" was a weak, parochial field of study. Under the thoughtful and imaginative leadership of its first chairman, Gabriel A. Almond, the Committee set about to revitalize the field in two ways: (1) to broaden the scope of investigation to include non-Western political systems, and (2) to move beyond the historical and descriptive treatment of formal institutions to analyses of "groups and interests that provide the dynamics of politics in different settings."[3]

The Committee was eminently successful in achieving these objectives. By 1960, the "revolution" had broken out and was sweeping the comparative field. Instrumental in bringing about significant change were the Committee members of the 1950s. Almond has suggested[4] that the time was ripe for innovation, but the Committee's accomplishments cannot be overstressed.

A turning point in the intellectual history of the Committee on Comparative Politics came in 1960, when Almond and James Coleman published *The Politics of the Developing Areas*. Here one could see—especially in Almond's praiseworthy introduction—the effort to bring non-Western societies within purview and to move away from the "formal and institutional bias" in

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**Crises and Sequences in Political Development*. By Leonard Binder, James S. Coleman, Joseph LaPalombara, Lucian W. Pye, Sidney Verba, and Myron Weiner. (Princeton, N.J.: Princeton University Press, 1971. Pp. 326. $8.00.)

The earlier volumes in this series, which have been reviewed previously in the *APSR* and will be referred to in this review essay, include:

*Communications and Political Development*. Edited by Lucian W. Pye. (Princeton, N.J.: Princeton University Press, 1963. Pp. 381. $6.50.)

*Bureaucracy and Political Development*. Edited by Joseph LaPalombara. (Princeton, N.J.: Princeton University Press, 1963. Pp. 487. $8.50.)

*Political Modernization in Japan and Turkey*. Edited by Robert E. Ward and Dankwart A. Rustow. (Princeton, N.J.: Princeton University Press, 1964. Pp. 502. $8.75.)

*Education and Political Development*. Edited by James S. Coleman. (Princeton, N.J.: Princeton University Press, 1965. Pp. 620. $10.00.)

*Political Culture and Political Development*. Edited by Lucian W. Pye and Sidney Verba. (Princeton, N.J.: Princeton University Press, 1965. Pp. 574. $10.00.)

*Political Parties and Political Development*. Edited by Joseph LaPalombara and Myron Weiner. (Princeton, N.J.: Princeton University Press, 1966. Pp. 487. $8.50.)

† The authors wish to acknowledge helpful criticisms and comments provided by Professors Harry Eckstein, Princeton University; W. Phillips Shively, University of Minnesota; Henry Teune, University of Pennsylvania; and three anonymous readers selected by the Book Review Editor. The authors, of course, assume sole responsibility for the contents of this essay.

[1] Leonard Binder et al., *Crises and Sequences in Political Development*, p. ix.

[2] Lucian W. Pye and Kay K. Ryland, *Activities of the Committee on Comparative Politics, 1954–1970* (New York: Social Science Research Council, 1971), p. 8.

[3] *Ibid.*, pp. 1–2.

[4] Gabriel A. Almond, *Political Development: Essays in Heuristic Theory* (Boston: Little, Brown and Co., 1970), p. 12.

comparative studies. By the time the volume had appeared, however, something more was needed. It had become apparent to the Committee that "without a more solid theoretical basis comparative studies of groups and leadership would only produce a proliferation of *ad hoc* research that would not be cumulative."[5] What was required was a theory of political development based upon a conceptualization of the whole political process changing through time.[6] Contributing to the development of such a theory thus emerged as a major objective of the Committee.

Almond's introduction to the first volume in the series shows that the Committee's attention was being directed toward the building of theory: " . . . the Committee hopes to make a contribution to political theory . . . The Committee's program seeks to develop concepts, insights, and theories which will improve our capacity to understand these experiments [new or rapidly changing nations] and to evaluate their prospects."[7]

Not only must *Crises and Sequences*—"a report on work accomplished"—be considered in terms of the Committee's objectives and intentions, but it must also be viewed in light of the resources available for conducting its programs. In terms of personnel, the Committee has been favorably situated. Between 1954 and 1971 (when *Crises and Sequences* was published), nineteen scholars had served on the Committee; of the ten Committee members in 1971, seven had served for a decade or more. Only two chairmen, Almond and Pye, had guided its activities. Thus the Committee has happily facilitated continuity through long tenure, and has sought fresh ideas and different skills through cooptation of new members at appropriate times.

Used to austere budgets, most political scientists would view the Committee's work as having been quite well financed. From January 1954 until March 1971, the group received about $774,475 for its work.[8] But this averages only about $45,000 per year. With this budget, the Committee sponsored 23 conferences, cosponsored six others, and conducted five summer workshops. Participating in these activities were 245 scholars from 67 different institutions; 45 of the scholars came from other countries. Most of the $200,000 spent directly on research by individuals went for a competitive grant program, supporting the field studies of 34 scholars in 21 countries.

While some might suggest that more funds should have been directly allocated for specific theory-construction objectives, there can be no doubt that the Committee was well-administered and that it generated a great deal of intellectual activity—a tribute to the two chairmen. The Committee's balance sheet is evidence of the piddling amounts available for attacking the difficult research problems in comparative politics. where expensive fieldwork overseas is essential.[9]

### Theory Development: The Approach in the Earlier Volumes

Since an evaluation of *Crises and Sequences* represents an appraisal, at least in part, of the Committee's research activities in terms of its goals in the 1960s, the volume cannot be examined in isolation. The six previous books, which are part of the research effort, must also be taken into account. Our concern in this essay is with the "quality of the published products"[10] *as they relate to the goal of theory development;* hence, in the earlier volumes we shall consider not the detailed, empirical subject matter but only the conceptual and theoretical issues that arose and bear upon the content of *Crises and Sequences.* Most important, we shall dip into the six books to see how the Committee approached the task of theory development, which will help to account for the kind of theory that emerges in the final volume.

In the period before it began the work on *Crises and Sequences*, the Committee apparently did not place much emphasis on the development of a conceptual structure and a body of coherent and interdependent propositions. Although the Committee held conferences on theoretical and methodological issues, the earlier volumes in the series were planned without benefit of rigorous designs and frameworks to which the contributors would be expected to adhere. The Committee members seem to have viewed such conceptual activities as being more appropriate at a later stage, after a body of empirical information had been collected and absorbed.[11] This orientation appears to have been dominant during the Committee's formative stage:

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[5] Pye and Ryland, *Activities of the Committee,* p. 2.

[6] This is implicit in the statement in Pye and Ryland, p. 2.

[7] Gabriel A. Almond, "Foreword," in Pye, *Communications and Political Development,* p. vii. The Forewords to the next two volumes contain similar statements. In *Political Development,* Almond points out that the Committee "took on the task of encouraging the development of theory which would enable political science to deal more adequately with the causation of political phenomena and with the variety of political forms" (p. 15).

[8] Pye and Ryland, *Activities of the Committee,* pp. 6, 70–71.

[9] We must, of course, recognize that some members of the Committee had access to funds for research that were not an official part of the Committee's activities. Some of the research supported by these funds has contributed to the theory-building work of the Committee.

[10] See Pye and Ryland, *Activities of the Committee,* p. 8. Another criterion is the impressive number of publications that emerged from the Committee's research activities. According to the authors, the Committee "helped in the production of 296 written reports, including books and articles in journals, unpublished research memoranda, and other documents."

> The committee did not question the possible usefulness of theoretical insights but considered ways in which their usefulness could be tested. It was suggested that a "forced draft" approach to theory construction often produces quite harmful results . . . The committee plans at a later time to give a more thorough consideration to the question of the applicability of general theoretical systems to the study of comparative politics.[12]

The first six volumes show that a case study of some aspect of political development in a specific country was the favored vehicle for accumulating and presenting the empirical information. Although the studies open up rich veins of factual knowledge, they are not usually employed as a device for testing hypotheses that emerge from a body of coherent theory. They are not even used to supply tentative answers to a set of common questions. Instead they are used to present diverse types of information as a basis for developing empirical generalizations. Rejecting the strategy of designing a rigorous framework to guide the preparation of the individual studies, the Committee chose to issue only general instructions and to give the authors relatively free rein so that they could use their imaginations and develop their materials according to their own preferences. Take, for example, the *fifth* volume (*Political Culture*), which apparently involved the most structured guidance up to that time—a "suggestive but not prescriptive memorandum." One of the editors points out the general approach:

> . . . it seemed best to emphasize more the existing richness of area studies than the potential advantages of systematic schemata for defining and classifying political cultures. . . . Some uniformity was requested of the authors as all were asked to include in their chapters some historical treatment and some evaluation of the significance of various socializing agents. . . . We are still convinced that the subtlety and richness of analysis guaranteed by providing latitude to the authors outbalances any returns that might have come from more restrictive but uniform instructions.[13]

He indicates that the country studies "were all initiated, and most were concluded" before Verba's instructive theoretical essay was available to the authors.[14]

Even the key concept "Political Development" was not given a precise definition which could be consistently employed. According to the editors of the sixth volume (*Political Parties*), "The term 'political development' remains elusive, and we have not attempted any systematic definition. Several preceding volumes in this series suggest preliminary conceptualizations, and it is our hope that the . . . concluding volume will provide a definition that confronts most of the conceptual pitfalls. . . . "[15] Imprecision in the central concept of the research means that the reader, as he proceeds through the first six volumes, encounters more than a dozen definitions of political development.[16]

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[11] A short conference on each of the first four volumes was held between early September 1961 and late June 1962. No specific conference was held for the volume on *Political Culture;* instead, the authors were given a "memorandum, suggestive but not prescriptive, which outlined subjects that might be generally relevant for the analysis of a political culture" (Pye and Verba, *Political Culture and Political Development,* p. vii). The planning conference for the *Crises and Sequences* volume was held in the summer of 1963. According to Pye: "Our focus at that time was on conceptualizing political systems, identifying their universal functions, and describing the processes of political modernization and development" ("Foreword," *Crisis and Sequences,* p. vii). In 1968, the Committee held another conference on the *Crises and Sequences* book, during which the preliminary work of the authors was discussed with other scholars.

[12] Gabriel A. Almond, "Research in Comparative Politics: Plans of a New Council Committee," *Items,* 8 (March 1954), p. 2.

[13] Pye, "Introduction," in Pye and Verba, *Political Culture and Political Development,* pp. 13–14.

[14] *Ibid.,* p. 14. The reader encounters in some of the other volumes similar statements concerning the objective sought and the research strategies employed. "[A] first measure in seeking intellectual order is to survey and appraise existing knowledge, and this has been a prime purpose behind our experiment" (Pye, *Communications and Political Development,* p. x). "As we are able to gather more information concerning the political forms, processes, and policies with which the newer nations experiment, it is hoped that our theoretical understanding of the dynamics of political change will be enriched and that, as a result, additional contributions can be made to the evolution of a genuine comparative political science" (Almond, "Foreword," in LaPalombara, *Bureaucracy and Political Development,* p. vii). "[An] endeavor of this kind can do little more than suggest the richness of thought and information that the varied papers of the authors represent" (LaPalombara, "Introduction," *Bureaucracy and Political Development,* p. 6). "Although no rigid uniformity has been sought in outlook or organization, the treatment is broadly parallel" (Ward and Rustow, *Political Modernization in Japan and Turkey,* p. 12). The editors of the introductory chapters in the volumes on *Education* and *Political Parties* introduce a more systematic, theme-oriented format, although not all of the contributors address the essential themes that were laid out.

[15] *Political Parties and Political Development,* p. 399. In his introduction to the fifth volume, Pye said: ". . . we have not tried to adhere to a rigid definition of political development. We have instead sought to incorporate in our analysis most of the dimensions of political change and modernization which students of the new states tend to refer to when speaking of political development" (*Political Culture and Political Development,* pp. 12–13).

[16] To document the various uses of the term in the several volumes would require a lengthy footnote. Pye lists some of the ways in which the concept has been defined in his introduction to the work on *Political*

In well designed research, the concepts are sharply defined and assigned labels so that the nature of the phenomenon being examined is clear from the outset. When the main concept lacks precision, individual authors are in effect invited to define it for themselves with the result that they will not be studying the same thing. To the extent that "political development" in this series is defined in different ways, it will be difficult—if not impossible—to relate the various propositions one to another. Many of the attempted generalizations in the first six volumes cannot be cumulative and will have to be redone when the concept is refined and a consistent usage facilitated.

Reworking some of the generalized statements will not be arduous because a few authors were careful to attach specific meaning to "political development" and they were able to make unambiguous statements about relationships among variables. Many contributors, however, did not define the concepts they were using, and while they sometimes asserted the existence of relationships, the reader often experiences difficulty in interpreting the meaning of the statements and in locating the evidence needed for the assertions.

Other types of unevenness in the six volumes also detract from their usefulness in theory construction. Some authors, whatever the questions they were addressing, carefully documented their reports; others, however, wrote impressionistically and speculatively. Some of the contributors specialized in fact-gathering—with or without classification schemes—and a few concerned themselves with historical chronology. Many of the studies suffer either from a paucity of quantitative data or from measurement problems. Moreover, since the data are typically cross-sectional, they cannot be used for the study of political development as a dynamic process. Statistics on school enrollments and curricula may provide information on exposure to the educational system but do not themselves constitute evidence on the importance of the school as a socializing agent in the absence of data concerning how effective the socialization experience actually was. A number of the studies list factors that "may influence" a particular phenomenon, sometimes noting that the list is not "exhaustive." But neither a listing of potential variables nor references to "explanation" are substitutes for theory construction.

We voice these criticisms not in order to blame but to show that since the questions and key concepts were not clearly defined in advance, one could hardly expect that a single volume, let alone the whole series, would be sharply focused and tightly integrated. It would be fortuitous, indeed, if the studies were cumulative. The scholars who passed judgment on the first six volumes in this *Review* criticized them in this regard.[17] Given the Committee's approach to theory building during the 1960s, however, these were not significant weaknesses. The studies in these early volumes were generally intended to be the raw material for concept formation and theory development. At this stage richness, variety, and broad coverage were preferred to rigor, consistency, and additivity. In other words, the Committee got what it wanted: the input for its theory mill. *Crises and Sequences* is the first major output from the mill. We shall now turn to an examination of this output. If it passes inspection, the criticisms of the earlier volumes are irrelevant; the Committee stands justified in the approach that it fol-

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*Culture* (pp. 11–12). He presents a good discussion of the concept and its diverse meanings in his *Aspects of Political Development* (Boston: Little, Brown and Co., 1966), pp. 33–45.

[17] The following are examples of critical comments of the first six volumes by reviewing scholars in this journal. *Communications and Political Development:* "[The essays] are in no way cumulative, and not always complementary" (Richard B. Fagen, *American Political Science Review,* 57 [September, 1963], 676–677). *Bureaucracy and Political Development:* "However, most of the other contributors [except Eisenstadt and Riggs] never stopped to ask what this volume was to be all about and thus confronted the editor with a difficult task indeed" (Alfred Diamont, *American Political Science Review,* 58 [December, 1964], 1032–1033). *Political Modernization in Japan and Turkey:* "Few of the chapters on the two nations make explicit comparisons, . . ." (Douglas H. Mendel, Jr., *American Political Science Review,* 58 [December, 1964], 1038–1039). *Education and Political Development:* "It would have helped had more precise definitions of different kinds of political elites been set forth, since they are used by the several authors differently—and even by the same author . . . The contributions are very uneven and do not present similar research results from different countries . . . The Committee . . . should consider being more selective and more restrictive as to focus next time around" (Edward W. Weidner, *American Political Science Review,* 60 [March, 1966], 121–123). *Political Culture and Political Development:* "There are rather fundamental differences in the conceptualizations . . ." (Aristide R. Zolberg, *American Political Science Review,* 60 [March, 1966], 119–121. Since the reviewer objects to the "premature achievement of theoretical monopolies," he regards the representation of different approaches and themes as a "major quality" of the book). *Political Parties and Political Development:* "[The] articles are special studies and not really additive theoretically . . . [The] concepts utilized are often unclear and the operationalization of key variables left to the imagination of the reader. There is no systematic empirical analysis attempted" (Samuel J. Eldersveld, *American Political Science Review,* 61 [December, 1967], 1166–1167). Most of these reviewers were more critical of these books than we have been. In fact, we regard much of the criticism as a bit premature, since the Committee, at this juncture in its activities, was explicitly in the preliminary stage of theory development which involved surveying data and seeking stimulating ideas and insights.

lowed. But if we find the output deficient, we shall then be interested in looking at the basic design of the mill and how it has been operated. Serious weakness in design and operation call into question not the content of the early works but the whole approach to theory development adopted by the Committee.

### Crises and Sequences: An Exposition

For a multiauthored work, *Crises and Sequences* has remarkable unity, demonstrating the advantages of close interaction and long tenure on the Committee. With the exception of the first and last chapters, the central theme of political development and the crises that emerge is laid out and elaborated in terms of several key concepts, used with striking consistency.

The crucial task of constructing a definition of political development was undertaken by James S. Coleman in Chapter 2:

> Our conception of the political development process is a continuous interaction among the processes of structural *differentiation*, the imperatives of *equality*, and the integrative, responsive, and adaptive *capacity* of a political system. The interaction of these three dimensions constitutes what we have termed the "development syndrome." Political development, in these terms, is seen as the acquisition by a political system of a consciously sought, and a qualitatively new and enhanced political capacity as manifested in the successful institutionalization of (1) new patterns of *integration* and *penetration* regulating and containing the tensions and conflicts produced by increased differentiation, and (2) new patterns of *participation* and resource *distribution* adequately responsive to the demands generated by the imperatives of equality. The acquisition of such a performance capacity is, in turn, a decisive factor in the resolution of the problems of *identity* and *legitimacy*.[18]

The first two technical terms in the definition are very clear. Differentiation refers to

> the process of progressive separation and specialization of roles, institutional spheres, and associations in societies undergoing modernization. The assumption has been that the more highly developed a political system becomes, the greater will be its structural complexity and the larger the number of explicit and functionally specific administrative and political structures it will have.[19]

Equality is treated as having three components: national citizenship, a universalistic legal order, and achievement norms.[20] As it is defined, capacity refers not only to the ability of a polity to "manage the tensions created by increased differentiation" and to "respond to or contain the participatory and distributive demands generated by the imperatives of equality," but also a "new and enhanced capacity to plan, implement, and manipulate new change as part of the process of achieving new goals." The main attributes of this concept "creative capacity" are *scope* and *effectiveness*, which are aspects of a developing polity's performance, and *rationality*, which is "historically and logically associated with the secularization of government, that is, with the *differentiation* of government processes and procedures from religious organization, influence, and control."[21]

The central theme of the volume emerges clearly in the Coleman chapter. As a polity develops or modernizes (i.e., increases in differentiation, equality, and capacity), certain crises are generated: identity, legitimacy, participation, penetration, and distribution. Starting at some position on a multidimensional scale, a polity has a given degree of differentiation, equality, and capacity; if the polity is stable, it will have a type and level of identity, legitimacy, participation, penetration, and distribution that are compatible with that position. But as the polity modernizes, the degree to which these five elements are present becomes inappropriate or their types incompatible with one another, and a crisis is precipitated in one or more of the "problem areas." The various potential crises are interrelated; one can give rise to another. This raises the question—not systematically addressed until the final chapter—of whether there may be sequences of crises.[22]

The analytical scheme, then, looks something like this: the elements of the development syndrome interact over time, and the incongruencies that emerge tend to generate particular types of crises. But if a polity has moved through the process, it is "developed" in the sense that it has new, enhanced capabilities.

Several questions arise as one ponders the scheme. If the development process involves continuous interaction among the three elements, is there not some measure of tautology built in because capacity is partially defined in terms of differentiation? What guidelines do we have for assessing the interaction—an increase in how much differentiation is equivalent to how much equality or capacity? This is a matter of some importance, since, according to the authors, the elements of the syndrome do not covary. An increase in "survival" capacity, for example, may require societal diffusion (de-differentiation) and/or a decrease in equality. In what specific ways are the five "crises" related to the components of the syndrome? From the viewpoint of theory, it would be helpful if there were hypotheses that stated *precisely* what types and degrees of change in what elements can be expected from particular types of incongruences.

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[18] *Crises and Sequences*, pp. 74–75. The notion of the "development syndrome" was first presented by Coleman in the *Education* volume, pp. 15–16.

[19] *Crises and Sequences*, p. 75. Coleman's definition of political development still leaves considerable ambiguity. If the political development process is an *interaction* among a class of processes, a class of imperatives, and a type of capacity, then the more highly developed a political system becomes, the greater will be this interaction. According to Coleman's formulation, the levels of the variables do not enter into the definition until later, and then in a manner that leads to a quite different definition. In fact, high political development as high interaction is never used in the book. Instead, greater political development is conceived of as greater differentiation, greater equality, and greater capacity. Why the authors should force the careful reader to explore the implications of the interactions and to sort out the intricate shifts between process-oriented and structure-oriented definitions perplexes us.

[20] *Crises and Sequences*, p. 77.

[21] *Ibid.*, pp. 78–80 (emphasis added).

[22] Significant subthemes are, of course, introduced by individual authors, but this summary catches the essence of the concern. Without this concern, there would be no reason for a book with this subject matter and title.

Rather than elaborating specific hypotheses, Coleman discusses each of the three dimensions of the syndrome as they apply in the developing areas. Differentiation is largely a problem of balance and mix; equality is the problem of mass expectations and demands; capacity is the problem of integration and performance. There is a considerable body of literature on the problems of differentiation and equality in the Third World, to which the author makes good reference. Capacity, however, is a trickier concept which, although widely used by students of development, has been little analyzed.

Moving smoothly from Coleman's contribution, Lucian Pye treats the identity crisis and the legitimacy crisis in Chapters 3 and 4. After distinguishing between mass political culture and elite political culture, he argues that a

> crisis begins to occur when the particular character of either the mass or the elite culture, or both, causes the inherent strains between, say, capacity and equality to become greatly magnified and perceived as a major threat to rulers or subjects or both. The result may then be that people question the basic unity of the society, causing an identity crisis, or they may question the proprieties of government, causing a legitimacy crisis.[23]

Pye lists four types of identity crisis: (1) those relating to nationalist feelings about territory; (2) situations in which social structural cleavages prevent effective national unity; (3) conflicts between identification with subnational groups (e.g., ethnic loyalties) and commitment to national identity; and (4) those deriving from the "psychological consequences of rapid social change and ambivalent feelings toward outsiders."

The author then presents a classification of elite cultures that are related to problems of national unity. In an *expanding elite culture*, the members of the political class are willing to share their standards and norms with the masses and to provide a meaningful place in the polity for all citizens who are prepared to accept the essential rules of the political game. This pattern for solving the national identity crisis provides a basis for coping with participation crises at a later time.

An *exclusive elite culture* attempts to establish a common basis for nationhood, but significant elements of the population (such as ethnic minorities) are excluded and relegated to uncertain status.

A *closed political culture* establishes two identities, one being related to the dominant elite culture and the other being linked with the mass culture.

In the *parochial elite culture*, the elite, while professing commitment to modernization, clings tenaciously to traditional values, denying leadership posts to those who possess the skills to carry out development programs.

In countries that have no dominant culture, there may be created a *synthetic elite culture* that "rests heavily upon the institutions introduced through foreign rule, but that must somehow be accepted as expressing a legitimate form of nationalism."[24]

Finally, there is a category that embraces countries *without a model elite culture*. In these cases no dominant culture existed historically, and the colonial power ruled directly without fostering the growth of a local elite and an indigenous culture.

Pye concludes his discussion of identity crises with a statement about the importance of leadership in "resolving identity problems and creating the basis for national unity." Given the perspective of the volume under review, this strikes us as being a type of capacity, but the author does not use the term in this context.

Pye's treatment of leadership takes him to an analysis of the legitimacy crisis, which he defines as a "breakdown in the constitutional structure and performance of government that arises out of differences over the proper nature of authority for the system."[25] Four classes of legitimacy crisis are identified: (1) conflicting or inadequate bases of authority, as when a ruling group cannot develop an acceptable rationale for enhancing its authority, or is forced to compete for power with other groups, such as a religious hierarchy or a tribal oligarchy; (2) excessive and uninstitutionalized competition for authority, typified by societies in which orderly governmental structures have not yet taken form; (3) rejection by the masses of the rulers' claim to legitimacy on the basis of their interpretations of history or their promises of "future accomplishment"; and (4) dysfunctional socialization processes.

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[23] *Crises and Sequences*, p. 104.

[24] *Ibid.*, p. 131.

[25] *Ibid.*, p. 136.

The key to the solution of any legitimacy crisis is *dynamic leadership,* which in turn requires an appropriate mix of institutional legitimacy and personal legitimacy. In seeking to establish their authority, leaders are often confronted with "basic dilemmas of choice." These include the need to remain firm in their adherence to traditional forms balanced against the need to accommodate to changing conditions; the question of preserving or rejecting the past; and the problem of responding to the immediate desires of the people or of persuading them to make sacrifices for future goals.

In summarizing his analysis of the identity and legitimacy crises, Pye indicates that he has in effect been discussing political alienation. He identifies two forms of alienation which arise from incongruities between the general and political socialization processes. The first type occurs when early childhood socialization creates an image of social relations as being orderly and just, but later experience in politics reveals a world that is "unregulated and dishonest." The second form of alienation—prevalent in contemporary developing societies—arises when early socialization fosters a suspicious and distrustful outlook upon the world, and agents of political socialization later present conflicting outlooks—some making exaggerated appeals to idealism while others portray political life as vicious and corrupt.

After formulating a broad definition of *participation,*[26] Myron Weiner, in Chapter 5, marks out several conditions under which a rapid rise in participation occurs: (1) periods of *social mobilization;* (2) change in the patterns of *social stratification,* as when the modernization process creates new social groups or modifies the relations among existing groups; (3) activity on the part of the *elite*—in, for example, nationalist movements or causes devoted to the spread of egalitarian ideas; intra-elite conflict may also lead to expanded participation as various factions compete for wider support; (4) changes in *government output,* illustrated by increases in government activity and the granting of suffrage rights, which bring new groups into contact with the rulers and broaden the arenas of political conflicts. According to the author, social mobilization and changes generated by the elite are related to equality, social stratification is associated with differentiation, and the government output factor is linked with capacity.

Weiner then looks at the growth of participation—and the timing of the growth—in relation to other changes in the political system. (1) In those countries where the *institutional framework for participation* grew out of the demand for participation, citizens are likely to value representative institutions. But where, as in many developing states, such institutions were created before participatory demands, popular commitment to the new forms may be shallow. (2) As citizens feel the impact of the *growth of central authority,* they try to increase their influence in national politics, and their new links with the center can undercut the local leadership. But if widespread participation occurs before the transfer of power from the local to the national level, the establishment of a representative system may enable local notables to extend their power to the national scene. (3) If political institutions are able to sink roots before *bureaucratic structures* grow overpowering, the people stand a better chance of making the bureaucracies responsive to their wishes. (4) Most Western countries developed a *sense of national identity* before the demand for participation arose. However, in many contemporary modernizing societies, where multiethnic groups are already participating to a high degree, the fostering of central loyalties is a much more difficult task.

A participation crisis is defined as "a conflict that occurs when the governing elite views the demands or behavior of individuals and groups seeking to participate in the political system as illegitimate."[27] Thus a crisis can occur when the ruling authorities refuse to share power with social groups whose demands or values they consider to be illegitimate, or when the elite does not approve of the mechanisms through which the demands are being made, or the methods that are employed. Sometimes the demands themselves may be regarded as being out of the question, as when a dissident group calls for secession, special corporate representation, or the transfer of power to a local level. Finally, a participation crisis can break out when the group seeking influence will not settle for the sharing of power with existing elites but is determined to replace these elites and then to deny them "the right to hold power."

Weiner next discusses the various ways in which governing elites respond to participation crises, the new institutions that may emerge from the resolution of a crisis, and the dilemmas facing new participants who seek to enhance their influence within the political system. The crises of identity and legitimacy, he points out, may be settled for relatively long periods of time; but alterations in socioeconomic patterns and generational change may lead to participation crises in recurring fashion.

In Chapters 6 and 7, Joseph LaPalombara treats the crises of penetration and distribution.

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[26] See below, pp. 988–989.

[27] *Crises and Sequences,* p. 187.

Both of these are related to capacity, but distribution is also associated with equality. He defines penetration as "conformance to public policy enunciated by central governmental authority. The degree of penetration in any polity may be viewed as the probability that governmental policies regarding the polity as a whole, or any of its subdivisions, will be carried out . . . "[28] The penetration phenomenon has two important dimensions: (1) the central government's capability of achieving it regardless of the attitudes of the citizenry; and (2) the ability and predisposition of citizens to "receive information regarding policy accurately and to *wish* to conform to such policies voluntarily."

The author's definition of "penetration crisis" is not as clearcut as his specification of "penetration."[29] Apparently a crisis is distinguished by "pressures on the governing elites to make institutional adaptations or innovations of a particular variety." Avoidance or adequate resolution of such a crisis requires that a government be concerned not only with popular attitudes but also with structures, employing a mixture of traditional forms and new institutions.

LaPalombara detects four types of penetration crisis, some of which may overlap: (1) *empty territory*, as when a regime attempts to push forward into new lands or to impose tighter central controls over localities within the present boundaries; (2) *regional differences*, typified by heavy contrasts in cultural and economic life between the center and outlying districts; (3) *communal autonomy*, as when the rulers decide, in the interest of national development, to change the status of a minority ethnic group; and (4) *peasant communities*, represented by a regime's efforts to extent its influence into the countryside.

The author, in Chapter 7, narrows the definition of distribution problems "to include those that imply that the political elite take a hand to increase the material goods available to a society or to redistribute such goods as may be available at any given time."[30] Either of these two policy situations can lead to a crisis at any stage of development, and the crises can recur.

The chapter explores the factors that influence the capability of political elites to deal satisfactorily with distribution problems—ideology, physical and human resources, and the international environment. In discussing the ideological factor, LaPalombara deals with nationalism (including the "competition for a constant pie" orientation); socialism; and "welfare capitalism"—the contemporary view that responsibility for solving distribution problems rests with the public sector.

On the issue of natural resource development, the author questions the belief, prevalent among economists, that an emphasis upon industrialization at the expense of agricultural development will invariably result in enlarging the economic pie. Urbanization in the developing countries, he warns, is characterized by pools of unemployed peasants and intellectuals whose demands upon the government might precipitate distribution crises.

Turning to human resources, he discusses the manpower problem in terms of the quality of leadership required to formulate and execute economic plans, and the psychological barriers to economic growth. The obvious answer to the problem of training managerial talent and of inculcating appropriate attitudes toward modernization among the population is "education." But here the author raises some intriguing policy questions which will challenge the leaders of developing countries. He suggests that "both the configuration and hierarchy of demands will differ from country to country . . . " and that "each situation must be treated in many respects as if it were . . . unique."[31]

When LaPalombara turns to the constraints of the international environment upon the elite's capacity to handle distribution problems, he decries the tendency of nation builders to want to imitate the West. Such a posture leads them to neglect agriculture, to uproot indigenous institutions that may be useful, and often to make unwise investment decisions in their haste to make their country appear "modern."

Compared with the crises of identity, legitimacy, and participation, distribution crises are less permanently resolved. Even the "mature" industrialized states face recurring crises of this type. In the Third World countries, however, the recurrence of the distribution problem may come while the rulers are still trying to resolve the identity question, the legitimacy crisis, or some other type of emergency. Having to face so many demands simultaneously places a burden on the political system which it may not be able to carry.

This, then, completes the description of the conceptual framework and the summary of the several components of the main theme, leaving for discussion the first and last chapters. Chapter 1, by Leonard Binder, is intended to be a general introduction to the entire book. It treats the development crisis through historical and philosophical discussions of stability and change and of the individual's role in society. Aristotle, Ibn Khaldun, Hegel, Marx, Rousseau, Sartre, and other eminent philosophers make their presence felt through Binder's erudite pen. The task of providing an expository review of this chapter we shall have to leave to those who are more competent than we. The essay appears to us to be ambiguous in many places, failing to define technical terms, to provide transitions needed to follow the argument, and to establish foreshadowing connections with the rest of the book. Consider the problems raised in the following paragraph:

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[28] *Ibid.*, p. 208.

[29] *Ibid.*, pp. 205–206, 227.

[30] *Ibid.*, pp. 235–236.

[31] *Ibid.*, p. 265.

> Fairer pricing, cr the greater efficiency of factor markets throughout the whole system, might be thought of in terms of the "integration" of factor markets. The view does not require that factor prices be the same throughout the entire geographical area coinciding with the economic system; it merely insists that price differentials be justified in terms of costs such as transportation, storage, financing, and the like.[32]

Does "or" in the first sentence equate "fairer pricing" with "greater efficiency" (alternative terms for the same concept), or does it mean that two different things ("fairer pricing" and "greater efficiency") may be thought of as elements in a single concept " 'integration' of factor markets"? Does " 'integration' of factor markets" mean the integration of the market for labor with the market for land and/or capital, or does it refer to integration with one another of geographically local markets for land, labor, and capital? From a reading of the previous page, we learn that this "theoretical use of the term [efficiency] still has practical consequences for the price structure and the availability of the factors of production throughout the economy, while its more analytical aspect is evident in the use of the alternative term 'integration'."[33] It might now be implied that, by looking at the greater efficiency of factor markets in terms of the "integration" of those markets, we gain a greater analytical perspective. Does thinking of "fairer prices" in terms of the "integration" of factor markets give us the same advantage? But what is this "more analytical aspect"? The author does not give us much help on these questions, for he digresses into some thoughts about how intriguing it is that the concept "integration" has terminological relevance for a number of the social sciences. Even if we were to concede that the statement makes sense, we cannot see what purpose it serves. Since there are no footnotes in this portion of the essay (nor in some of the other crucial sections) to indicate what literature the author is referring to, we are left floating adrift.

The concluding chapter by Sidney Verba is an entirely different matter.[34] In many respects it is a commentary on the other chapters in the volume and on the work of the Committee on Comparative Politics to date. Verba's chapter, which is an admirable piece of "Committee self-criticism," could have been published separately as an analytical overview. We shall be referring to it frequently in the analysis that follows.

### Crises and Sequences: A Critical Analysis

According to the foreword, *Crises and Sequences* is "noteworthy as an effort at collective theory building in the social sciences."[35] The approach taken is what we would call *intuitive empirical generalization.* In this approach, there is little emphasis during the early stages on rigorous concept formation, systematic analysis, or the development of interrelated propositions. Information and ideas are needed first, and impressions, speculation, and some systematically collected data are gathered in. Insightful scholars then reflect upon these materials, attempting to develop general concepts and conclusions which will "make sense" out of the rich diversity of human experience. The process by which one moves from the raw material to the theory is never made explicit. Rules of inference are not spelled out. Intuitive processes are apparently considered to be the most important.[36]

This approach may be contrasted with two others—*systematic empirical generalization* and an *analytic-deductive approach.* In systematic empirical generalization, more attention is paid initially to concept formation and particularly to operational definitions, but general propositions are developed from an examination of empirical data with rules of inference being made explicit at the outset. Theory-building efforts that rely heavily upon inferential statistics are good examples.[37] Since quantifiable data are highly desirable if the theory builders are to express the relationships among variables systematically and precisely, attention must be given in the early stages to problems of measurement and to the development of measuring instruments.

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[32] *Ibid.*, p. 25.

[33] *Ibid.*, p. 24.

[34] The essay was originally written as "an internal memorandum to alert the other authors" to certain conceptual problems. (See Pye, "Foreword," in *Crises and Sequences*, p. ix.)

[35] *Ibid.*, p. ix.

[36] Besides utilizing their own research for the impressive knowledge about the developing areas which they display, the authors draw upon the Committee's earlier works in formulating the empirical generalizations that emerge in *Crises and Sequences.* The book on *Political Culture,* which has implications for identity, legitimacy, and participation problems, is an important antecedent of the *Crises and Sequences* volume; in the wrapup commentary, one of the editors treats the developmental crises and presents a short discussion of the question of sequencing. *Political Parties and Political Development* obviously deals with political participation; but it is also relevant to the treatment of legitimacy and penetration. The volume on *Bureaucracy* is clearly related to problems of penetration, but it is not used in this context as explicitly as one might have expected. *Education and Political Development* is also concerned with penetration, but its heavy emphasis on socialization makes a good portion of the content relevant to the analysis of identity and legitimacy problems. As we have already indicated, an early discussion of the development syndrome appears in this work. *Political Modernization in Japan and Turkey* bears upon all of the crisis areas, and was employed particularly in the discussion of the identity problem. The first volume in the series, *Communications and Political Development,* contains material that influences ideas on penetration, integration, participation, and—to the extent that communications processes affect "rising expectations"—distribution.

An analytic-deductive approach to theory building stands in marked contrast to the other two. Its initial emphasis is on deduction rather than induction. At some point early in the theory-development process, careful attention must be paid to the specification of certain concepts as theoretical primitives in terms of which all other concepts will be defined. It is also important to specify the propositions that will be treated as axioms, and to identify the logical system (rules of deduction) which is being employed. In the subject matter of political science, William Riker's *Theory of Political Coalitions* and Mancur Olson's *The Logic of Collective Action* are examples of this approach.

There should be little argument that a body of precise, rigorous, and general propositions with real deductive power is a *sine qua non* of a science of politics. We assume that this is what Verba has in mind when he speaks of the goal of "a coherent and interdependent set of propositions."[38] Major disagreement exists, however, about how to achieve it.

We seriously doubt whether the intuitive empirical generalization approach taken by the Committee, as represented by these seven volumes and other published works, is either efficient or effective in realizing this scientific goal. After seventeen years of prodigious effort, dozens of commissioned and financially supported articles, six major volumes, and 284 pages of exposition in *Crises and Sequences*, we find Verba raising four basic questions:

I. What is a sequential model?
II. Sequences in relation to what?
III. Sequences of what?
IV. What is the relationship among the items in the sequence?[39]

It seems to us to be an inversion of the proper order that these four questions are raised at the end of the book. In our view, they should have been raised in 1963, when the volume was conceived so that the authors of the central chapters could have addressed themselves to the issues.

This objection suggests an even more basic question: If a theory of political development is the goal, why did the Committee choose the approach of intuitive empirical generalization, which places so little emphasis on stating a coherent and interdependent set of propositions with rigor and precision? Verba provides one answer:

> In most social research that attempts to deal with problems as complicated as "nation building," we usually face a dilemma. We can use fairly precise models that do not fit the empirical material or fairly loose formulations that fit reality better, but only because they are loose.
>
> . . . [A] sequential model may represent a loose framework for narration in which case it is more useful than no framework at all—but not much. Or it can be a more precise model of social change—in which case it would be terribly useful if we could achieve it, but quite *hard to achieve*. The rest of these remarks will be based on the assumption that we aspire to the latter model, but *are often forced*, in fact, to operate closer to the former.[40]

This paragraph raises several questions. Is developing a "loose framework" (through the approach taken by the Committee) in fact easier than developing a "precise model"? Why, if we aspire to the latter, are we *forced* to work with the former? Is it because working with a loose framework is a way of developing precise models?[41]

Our answers to these questions, we suspect, will differ considerably from those of the Committee. In order to determine whether one approach is inherently easier than another, let us compare with *Crises and Sequences* an important monograph that derives a precisely formulated, coherent, and interdependent set of propositions in an analytic-deductive approach—namely, Kenneth J. Arrow's *Social Choice and Individual Values*.[42] Arrow's book was published in 1951, three years before the Committee on Comparative Politics was organized and twelve years before *Crises and Sequences* was conceived.

Let us see first how the two works handle the problem of defining the key concepts. In his chapter in the Committee volume, Weiner defines political participation as "any voluntary action, successful or unsuccessful, organized or unorganized, episodic or continuous, employing legitimate or illegitimate methods intended to influence the choice of public policies, the administration of public affairs, or the choice of political leaders at any level of government, local or national."[43]

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[37] Other examples would include systematic comparative research and even case studies in which the strong inferential principle is employed.

[38] *Crises and Sequences*, p. 283.

[39] *Ibid.*, p. 285.

[40] *Ibid.*, p. 286 (emphasis added). *Model* is the key term in Verba's chapter; it is used ten times on this page alone. But we must cenfess that we are still not sure what the author precisely means by the term. It is clear, however, that a model is a *very good thing*.

[41] See *ibid.*, p. 283. We assume that by a "precise" model of social change Verba has in mind "a coherent and interdependent set of propositions."

[42] Kenneth J. Arrow, *Social Choice and Individual Values* (New York: John Wiley and Sons, 1951).

In arriving at this definition, Weiner reviews ten ways in which participation has been defined or treated in a large body of literature. He wanted to make sure that his definition would cover a "wide range of activities viewed as participation in different societies" and (at least implicitly) that his work would relate to that of other scholars who have analyzed the same phenomena. The formulation of this definition, we submit, was not easy. Besides the literature review, Weiner devoted considerable intellectual effort to distilling the various elements for his definition.

In Arrow's monograph, the concept "social welfare function" is as central as "participation" is in Weiner's chapter.

> By a *social welfare function* will be meant a process or rule which, for each set of individual orderings $R_1, \cdots, R_n$ for alternative social states (one ordering for each individual), states a corresponding social ordering of alternative social states, $R$.[44]

This should be an acceptable example of what Verba would consider a precise definition. But was it more difficult to formulate than the looser one developed by Weiner? Arrow based his definition on the work of one other scholar, and although he was familiar with alternative formulations, he made no attempt to incorporate them into his definition, nor did he examine what is meant by social welfare function in a wide range of societies. In this sense, Arrow's task was easier than Weiner's.

In the earlier sections of his work, Arrow was careful to attach precise definitions to such terms as *social states* and *individual orderings* for alternative social states. Later he provides clear-cut definitions of certain subtypes of social welfare functions, such as *imposed* and *dictatorial*.[45] Weiner, too, further explicates key terms in his definition, like *action*, *voluntary*, and *choice*. Although for the most part his terms are not as rigidly defined as Arrow's, we cannot see how the task he set for himself was easier—different, yes, but not easier.

If the crucial job of defining concepts is not any easier in a loose formulation than in a more precisely developed approach, let us see whether the comparison holds up with respect to the *hypotheses* that are developed. For this purpose we can look at two propositions submitted by Pye and one presented by Arrow. Pye's formulations are as follows:

> If there is no legitimacy crisis and the people are prepared to trust their leaders, then there can be a broad acceptance of the right of the government to do more than merely process and aggregate into public policies those interests articulated by the public at large . . .
>
> On the other hand, when there is a serious legitimacy crisis, particularly if it is related to a rise in the demands for equality, then the popular reaction may be that the elite lacks the authority to go beyond meeting the immediately articulated interests of the public.[46]

These are examples of propositions that Verba would consider to be widely applicable and loose in the sense that they are not tightly linked with other propositions in a coherent set. The first proposition, however, is precise. It states that if two conditions are met—(1) there is no legitimacy crisis, and (2) people are prepared to trust their leaders—then it is *possible* ("can be") that a particular consequence will come about: "a broad acceptance of the right of government to do more than merely process and aggregate into public policies those interests articulated by the public at large." Hence, if a single situation can be found where conditions (1) and (2) are met and where the consequence is also found, then the possibility will have been demonstrated and we could accept the statement as being empirically true. It is a statement that can be confirmed by a single case but can only be falsified by an investigation of every case. (Note that neither the necessity nor the high probability of the consequence is stated in the proposition.)

The second proposition states a single condition linked with a consequence by the verbs "may be." The statement has what Verba considers the virtue of being widely applicable, but it has this quality because of the ambiguity of the verbs "may be" in the formulation. Can any proposition employing this combination of verbs ever be falsified? Does "may be" denote anything different from its negation "may not be"?

Mr. Jones asserts, "John may be home at 1:00 A.M. on December 1, 1975." Mr. Brown goes to John's home at the stated time only to discover that John is not there. Has the proposition been demonstrated to be false? Would it have meant anything different if Mr. Jones had said, "John may not be at home . . . "? In everyday parlance, "may be" sometimes connotes a probability statement, i.e., more likely than not (probability $>0.5$). Since in scientific discourse there are such conventional ways of making probability statements, we assume that Pye does not wish to convey any specific probability range.[47]

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[43] *Crises and Sequences,* p. 164. We assume that most readers will agree that this definition is "loose" in the sense in which Verba uses the adjective.

[44] Arrow, p. 23.

[45] *Ibid.,* pp. 28, 30.

[46] *Crises and Sequences,* p. 155.

While criticizing the second proposition on the basis of its ambiguity, we do not wish to imply that either of the two statements was easy to formulate. Obviously, Pye has invested an enormous amount of time and thought in studying problems of authority and legitimacy. The effort, however, was devoted to learning about problems of legitimacy in many different political systems and to achieving wide applicability in the formulations; there was much less concern for precision, rigor, and the interrelationship of a set of propositions.

The most important proposition in Arrow's work is the general possibility theorem.

> If we exclude the possiblity of interpersonal comparisons of utility, then the only methods of passing from individual tastes to social preferences which will be satisfactory and which will be defined for a wide range of sets of individual orderings are either imposed or dictatorial.[48]

Unlike any of the propositions that might be extracted from *Crises and Sequences*, the statement by Arrow has been formally proved through the use of axioms and definitions (previously provided) and mathematical logic. The formulation is precise, and it is tightly connected to a number of other propositions by means of the deductive system employed. In other words, it is one proposition in a coherent set of propositions.

It would be meaningless to argue whether formulating the general possibility theorem was easier or more difficult than developing Pye's hypotheses. But we would contend that it is much easier to build a scientific theory of social choice and individual values from Arrow's work than to build a scientific theory of political culture and legitimacy from Pye's. The main reason is, of course, that the approach taken by the Committee does not provide for the coherent and interdependent set of propositions which is Verba's desideratum in a scientific theory, whereas the route that Arrow takes does. Furthermore, constructive criticism, disproof, and additive work are much easier in the Arrow approach. For example, Blau pointed out an error in the proof of the general possibility theorem; this was recognized and accepted by Arrow and other scholars, and the necessary modifications were made. We submit that it would be an immensely difficult task to detect flaws in the reasoning and analysis that led to Pye's conclusions (or the authors' conclusions in the other chapters).

We would not make so much of this issue were it not that Verba implies that the intuitive empirical generalization approach will over time facilitate the hammering out of a coherent and interdependent set of propositions that will explain political development. Indeed, one cannot justify the Committee's allocation of resources, given its goal of theory development, without this assumption. Rigorous deductive methods, we contend, are necessary for the theory building which the Committee has set for its goal. But, as reflected in the seven volumes under discussion, the Committee did little in the period from 1963 to 1970 to facilitate the development of theory that has coherence and interdependence, and hence deductive power.[49] Is it easier to specify theoretical primitives in 1975 that it was a decade ago? Is it easier now to designate a set of axioms? Is it easier now than it was earlier to identify the "rules of deduction" that would be the most appropriate? We think that the answer to all of these questions is "No." Thus, it really makes no difference which of the two approaches is the easier. If the development of the loose formulation does little or nothing to further the development of tight, coherent, and interdependent sets of propositions and that is the goal, then why waste resources on such formulations?

While what we have said indicates our disagreement with the Committee on the most efficient way to develop a science, we nevertheless commend Verba for exploring the structure of sequential models.

> . . . The resultant condition that one is trying to explain (the existence of democracy, the development of a "modern" state, the growth of a bureaucracy) is explained in terms of a number of initial conditions. Furthermore, the initial conditions are not necessarily treated as if they occur simultaneously. The crucial concern is with the effect on the resultant condition of the order in which the initial conditions occur—this may include simultaneous occurrence, but that

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[47] We should call attention to the fact that a number of these "may be"-type propositions can be found in the other volumes. An example of another form of proposition that is difficult to falsify is the following: ". . . the higher the degree of differentiation of group and other relationships is in a political system, the greater is the probability for the development of political parties, though this probability may be reduced by the presence of other impeding conditions." (William N. Chambers, "Parties and Nation-Building in America," in LaPalombara and Weiner, *Political Parties and Political Development,* pp. 83-84.)

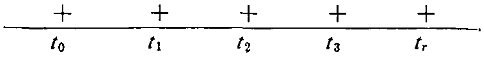
[48] Arrow, p. 59. All technical terms (e.g., "imposed," "dictatorial," and "satisfactory") are precisely defined.

[49] The references cited by the authors of *Crises and Sequences* reveal a secondary but interesting point. Literature that is relevant to the subject matter but employs an analytic, deductive approach is ignored. We find it difficult to imagine, for example, a chapter on political participation published in the 1970s that does not even refer to Mancur Olson's *The Logic of Collective Action* or does not address itself to some of the analysis set forth in Anthony Downs's *An Economic Theory of Democracy.*

is only one possible pattern. It is this stringing out of conditions over time that makes the model a sequential one.[50]

Verba's formulation, however, strikes us as somewhat elliptical. Why not simply use the classic statement: "The state of the system at some future time $t_r$ (resultant state or condition) is a function of its state at some earlier time $t_0$ (initial state or condition), plus all of the boundary events in the time interval $t_0 - t_r$"? If the initial conditions do not all occur initially, it is a bit confusing to view them as initial conditions; they are intermediate system states or boundary events and should be treated as such. In Figure 1, for example, Verba would be inclined to treat system states $t_0$, $t_1$, $t_2$, and $t_3$ as initial conditions occurring in a sequence. (If this phrase means anything, it must be that the time is collapsed.) The alternative is to explain system state at $t_1$ in terms of system state at $t_0$, plus boundary events in the time interval, and so on, until system $t_r$ is accounted for. Since there can be different system states at each time point—depending upon initial condition, boundary events, and the probabilities assigned—different paths can be identified.

**Figure 1**



This conceptualization seems to be what Verba is advocating on page 288: "Sequential models link a series of conditions or events stretched out over time. In this sense, a sequential model makes the dependent variable of the first proposition the independent variable of the next. One has a series of statements about the relation over time of $A$ to $B$, and $B$ to $C$, and so forth." This statement suggests a first-order difference equation, and we are somewhat puzzled why this "logical form" was not suggested rather than that of the time-series version of causal modeling which Verba advocates.[51] The first-order difference equation seems to have all of the analytical characteristics that Verba wants, as well as some very desirable features that will interest him. For example, he is not limited to first-order relationships; he can easily shift to a continuous form (differential equations), if this would be more appropriate, without destroying the basic structure of the analysis; and he can address formally the questions of path stability and robustness and of equilibrium. We are not necessarily advocating the use of differential equations as a language (including rules of deduction) for both stating and analyzing sequential theories; we are suggesting that they are superior to some modification of causal modeling in dealing with the problem as Verba defines it.[52]

In either approach it is necessary, of course, to devise a classification system which will handle different types of initial conditions. Since most efforts to build theories of political development begin with a category called the "traditional" political system, this means that the different types of traditional systems have to be specified. The Committee on Comparative Politics seems to have given little attention to this task, the essential nature of which should have been obvious by 1963. Some of the literature on political development suggests ways of attacking the problem; Apter's work is a notable example. In *The Political Kingdom of Uganda*, he presents a fourfold classification based on two types of authority structure and two types of value structure, yielding four types of "traditional" political systems, or "initial conditions." Apter is interested in the case in which the boundary event (Western colonialism) is the same for different types of initial conditions. If a society is to modernize, then a mobilization regime is likely if the initial condition was pyramidal authority and consummatory values; a modernizing autocracy if the initial condition was hierarchical authority and instrumental values; a consociational regime if the initial condition was pyramidal authority and instrumental values.[53] These, of course, can be treated as alternative paths to modernity. But, with further elaboration, such as

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[50] *Crises and Sequences*, p. 287.

[51] *Ibid.*

[52] Rather than attempting to adapt the Blalock causal analysis technique to longitudinal data, we would suggest going directly to Sewall Wright's method of path coefficients. While employing a logical structure comparable to causal analysis, Wright's technique was developed especially for the analysis of evolutionary phenomena. His original presentation was published in the 1920s. (See Sewall Wright, "The Relative Importance of Heredity and Environment in Determining the Piebald Patterns of Guinea Pigs," *Proceedings of the National Academy of Science*, 6 [June 15, 1920], 320–332; "Correlation and Causation," *Journal of Agricultural Research* 20 [January 3, 1921], 557–585.) For further elaboration, see Wright's "The Method of Path Coefficients," *Annals of Mathematical Statistics*, 5 [1934], 161–215, and "The Interpretations of Multivariate Systems," in *Statistics and Mathematics in Biology*, ed. Oscar Kempthorne, Theodore A. Bancroft. John W. Gowen and Jay L. Lush (Ames, Iowa: Iowa State College Press, 1954), pp. 11–34. An interesting discussion is contained in John W. Tukey, "Causation, Regression, and Path Analysis," in Kempthorne, pp. 35–66. John V. Gillespie has already explored the use of both the Blalock and the Wright techniques for the analysis of temporal sequences. He detects certain advantages in the Wright approach for the comparative analysis of time-series data. (See John V. Gillespie, *The Application of Causal Models to Comparative Political Analysis* [M.A. thesis, University of Minnesota, 1966], pp. 131–182.)

[53] See David E. Apter, *The Political Kingdom in Uganda: A Study in Bureaucratic Nationalism* (Princeton, N.J.: Princeton University Press, 1961), pp. 85–91.

Apter provided in some of his later works, it is possible to introduce refinements, e.g., subtypes of the basic regime categories and intermediate system states. The important point is, however, that the different initial conditions and intermediate regime types would seem to give rise to different sorts of identity, legitimacy, participation, penetration, and distribution problems and in different sequences. It is a pity that important questions like these, which Verba introduces in the concluding chapter of the seventh volume, did not attract the attention of the Committee more than a decade ago when *Crises and Sequences* was in the planning stage.

Any sequential (or evolutionary) theory must deal either with the basic dimension(s) to which the sequences will be related, or to some end state or resultant condition. The development syndrome provides the basic dimensions, or—if "modern" is considered to mean high equality, high differentiation, and high capacity—the resultant condition. Verba and some of his colleagues suggest that of the three elements in the syndrome, capacity is the most fundamental. It is certainly crucial to many of the approaches to the analysis of political development, and especially to one that emphasizes crises. The capacity to cope with crises and to resolve them must eventually be part of an adequate explanation of development.

In spite of the centrality of this concept, however, it is not defined with clarity and precision, as we pointed out on page 983 and footnote 19. In fact, the definition used in *Crises and Sequences* shows little refinement beyond that given in Coleman's earlier writing. We recognize that the problem facing the authors is difficult, but after seeing how they have struggled with the conceptualization of capacity, we venture to make what may appear to be a startling point: Capacity is a characteristic of a *formal theory* of empirical phenomena, not a characteristic of the phenomena themselves.

Some illustrations will help to clarify our position. Although the terminology is a bit strange, we can speak of the perfect market as having the capacity to allocate resources Pareto-optimally if external (dis)economies, increasing returns, public goods, and indivisibilities are absent from the decision environment. That the perfectly competitive market has this capacity can be proven in a formal sense, but it is the capability of the market in the theory, not of any empirical market economy.

We could also speak of the horsepower rating of a gasoline internal combustion engine as its capacity (not to be confused with displacement capacity). Rated horsepower is computed from a formula that takes into account the diameter of the cylinder, length of stroke, compression ratio, etc. If an individual hooked an engine to a dynamometer and discovered that its maximum performance was below the rated horsepower, would he conclude that its *capacity* rating (horsepower) should be reduced? Probably not. One would say that the engine was not performing up to capacity. But such a statement requires a conceptualization of capacity that is independent of measures of performance.

The usefulness of concepts like "capacity," however, is not dependent upon the researcher's ability to measure performance in the same terms, as is the case with the engine on the dynamometer. We are not aware of any study by an economist to explore empirically through direct measurement the question of whether a real market economy was allocating resources Pareto-optimally. (Indeed, we are uncertain whether the practical measurement problems of such a study could be solved.) Useful analyses of suboptimal resource allocation in the presence of monopolies and monopolistic price-setting *are* undertaken, but only after "optimal" has been precisely defined.

The work of the Committee on Comparative Politics has not facilitated the development of the type of analytical concept represented by capacity because such a concept requires a theory with deductive power. The Committee, as we have seen, has not yet moved to an approach that would make this objective realistic. Verba's extended discussion about the difficulty of measuring capacity is, we think, putting the spotlight on the wrong problem.[54] Given this context, one does not measure capacity; one seeks to measure performance, and then to compare performance with capacity.

In its original form, the conceptualization of the five crises also presents some difficulties. Putting aside for the moment the question of what sorts of phenomena these really are and how they are related to the components of the "development syndrome," we can detect the rudiments of a classification structure which has five different categories. The problem is that the categories are not defined with sufficient sharpness, and there is considerable overlap. The authors frankly admit that the categories are not mutually exclusive, although the possible overlaps do not seem to disturb them. But the fact is that analysts using this scheme would have the very difficult task of deciding whether a particular crisis fits into the category of "identity" or "legitimacy," or is better suited in the "legitimacy" classification rather than that of "participation," etc. The editors of *Political Parties and Political Development*, for example, conclude: "The earliest participation crisis may thus also involve a crisis in legitimacy."[55] Pye makes the statement: "The Russian, Chinese, and French revolutions all changed the basis of legitimacy in their respective countries but none were legitimacy crises as defined here because all were more centrally crises of participation and distribution. Some interpretations of the American revolution would make it essentially a legitimacy crisis, while other interpretations would make it too a participation and distribution crisis."[56] The authors never really tell us how to decide what kinds of crises one is actually observing.

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[54] *Crises and Sequences,* pp. 292–294.

In the latter part of his chapter, Verba clarifies the situation by seeking to refine the concepts in an important and useful way. Expressing a preference for "problem area" rather than "crisis," he then defines a crisis as a "situation where a 'problem' arises in one of the problem areas (that is, members of the society are discontented with one of the five aspects of the decisional process), and some new institutionalized means of handling problems of that sort is required to satisfy the discontent."[57] This conceptualization could lead to the identification of discontinuities which in turn makes a "stage" theory more attractive. Had this refinement been considered earlier, it too might have led to tighter specification in those chapters that deal with substantive matters.

Another difficulty—largely overlooked by the authors—arises from their concern about bridging the gap between the level of microanalysis (the individual) and the level of macroanalysis (the collectivity). In his introduction to *Political Culture and Political Development*, Pye expresses the hope that the study of individuals and of total systems can be brought together.

> There has been a need to discover a method for working back from the complex subtleties of individual psychology to the level of the social aggregate which is the traditional plateau of political science. It is this problem of aggregation—which involves the adding up of the discoveries of individual psychology in such a manner as to make community-wide behavior understandable in the light of individual actions—that is now the great challenge to political science and for which the concept of political culture holds such great promise.[58]

The shift in analysis from the individual level to the group level becomes especially marked in discussions of the "identity crisis." Some of the authors, influenced by the work of Erik Erikson, view the question of national identity as "the political culture version of the basic problem of self-identity." Individuals, for example, undergo a (political) identity crisis. (Am I a Virginian or an American?) And through some mysterious process this is translated into an identity crisis at the national level. But surely we need stronger scientific underpinning before we can speak of a *polity* as undergoing an identity crisis. The need here is for some rules that enable us to move from the psychological predispositions of individuals to a generalization about a pattern of psychological predispositions that characterize a reified entity like the nation-state. Pye sees the concept "political culture" as a way of relating micro phenomena to macro phenomena, but much remains to be done before this important objective can be realized scientifically.

Another difficulty involved in the level-of-analysis problem is suggested when the authors, in handling their nation-state materials, discover variability among geographic regions within the nation-state. One section of the country, for example, may be modernized, while another may still be under the strong influence of tradition. In some research problems, these differences among geographic areas may be greater than the differences between particular nation-states. The level of analysis at which research is to be undertaken will, of course, depend upon the nature of the phenomena being studied. In the study of some aspects of political development, a rigid focus upon the nation-state may obscure important variation at the local or regional level, which may reflect political change more sensitively.

Throughout this critique, we have not discussed the effort of the Committee to develop a capability for prescription. As was true in the earlier volumes, many assertions made by the authors of *Crises and Sequences* take the form of prescriptions. But the members of the Committee seem to recognize no major difference between a theory that will explain empirical phenomena (sometimes called "positive theory") and a theory that will guide the design of institutions and policies (sometimes referred to as "normative" or "design theory"). We argue that these are really distinct and that the failure to recognize the difference makes for deficient theory of both kinds.[59]

### A Final Word

Our critical remarks about the efforts of the Committee on Comparative Politics to make progress toward the construction of a theory of political development should not obscure the significant contributions the Committee has made to the discipline. Quite apart from the useful information provided in the case studies, the several volumes contain many explicit or implicit propositions which will be challenging to researchers. We have already mentioned how the Committee has encouraged scholars to move away from traditional perspectives. Equally important, it has expanded the context of investigation into non-Western areas of the world, making Western political scientists realize that many of their generalizations are extremely limited in scope, inapplicable to the developing areas without considerable revision.

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[55] LaPalombara and Weiner, *Political Parties and Political Development*, p. 18. See also p. 224.

[56] *Crises and Sequences*, p. 136.

[57] *Ibid.*, p. 302.

[58] Pye, *Political Culture*, p. 9.

[59] Some of the important differences are discussed in Herbert Simon, *The Sciences of the Artificial* (Cambridge, Mass.: M.I.T. Press, 1969).

But this list of accomplishments does not alter our basic point: given the objective of theory building—a coherent and interdependent set of propositions in terms of which political development can be explained—we feel that the Committee's work has fallen short of the target. Verba points out that the chapters in the concluding volume present a "framework for the study of political development, not yet a theory; . . . "[60] After such a long-term effort, we have only a framework—and one that still does not satisfy Verba as he perceptively highlights important weaknesses. But even he appears to recognize only the symptoms of the crises in theory development; he does not identify the basic reasons for the weakness—the way the Committee defined the sequence of intellectual activities in the collective theory-building effort.[61]

While being critical of the work of the Committee since 1960, we single it out only because we were invited to review its works. In fact, many of the attempts at theory building by political scientists would be subject to the same type of criticism. Many political scientists confronting the task of the Committee in 1960 would have approached it in the same way. The emphasis in the profession is on building intuitively attractive frameworks for organizing data, not on identifying significant theoretical problems (or puzzles) and presenting the propositions in terms of which these problems can be resolved, even though the propositions may turn out to be empirically false. When propositions are developed, those that can be easily confirmed but are difficult to falsify are preferred to those that are easy to falsify but difficult to confirm. Many scholars exhibit little interest in elaborating the reasoning that lies behind the development of a proposition; persuasive rhetoric seems to be preferred.

These tendencies seem to us to reflect the reward structure of the discipline. Scholars soon learn the "common law" of research propositions: to be trivial but true is a virtue if the alternative is to be meaningful but false. It takes no detailed history of science to point out the crucial instances when science took a major step forward as the result of a proposition being disproved and the theory on which it was based collapsing. The phlogiston theory and the ether theory, for example, could only be cleared away and something better developed if unambiguous, falsifiable propositions were advanced, with the reasoning behind them being well articulated. We have our phlogiston and ether theories in political science but they are partially hidden—implicitly embedded in grand but loose frameworks. They cannot be cleared away until they are made explicit and tested. But this activity is not rewarded—in terms of money or glory—when compared to the work of presenting a loose framework for organizing data. Had the members of the Committee set out in 1963 to develop a precise and rigorous set of coherent propositions which they (or others) tested and found to be false, the Committee's work would probably have been dismissed, if not derided. Thus, while we are critical of the Committee's approach and the published products, we attribute responsibility much more widely—to the practices and norms of the profession as a whole.[62]

Even though we have crossed lances at times with the authors of *Crises and Sequences*, we do not wish to be dogmatic about our position, and certainly we have no intention of directing an argument *ad hominem*. On the question of what is the most efficacious way to build theory, we hold open the possibility that we might be wrong. But we are sincere in our belief that progress in the development of a science of political life has been hindered by the absence of criticism and controversy over important, well-defined issues *among those who are dedicated to the scientific enterprise*. The history of science is replete with examples of advancement that came about as a result of intellectual disputation among adversaries. We share with the members of the Committee the goal of developing a scientific theory of political development. We hope that our critique will give rise to serious discussion which will contribute to the achievement of that goal. For our part, we approach the disputation in the spirit suggested by the Bard of Avon:

> Do as adversaries do in law,
> Strive mightily, but eat and drink as friends.[63]

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[60] *Crises and Sequences*, p. 283.

[61] We recognize that the position we have taken in this essay represents a minority view in all of the social sciences except economics. For a criticism—very much like our own—of the state of research efforts in psychology, see Paul E. Meehl, "Theory Testing in Psychology and Physics: A Methodological Paradox," *Philosophy of Science*, 34 (June, 1967), 103–115.

[62] We wish to acknowledge a special debt to Professor Harry Eckstein for contributing to our thoughts on the reward structure of the discipline.

[63] *The Taming of the Shrew*, Act I, Scene ii.

# BOOK REVIEWS

**Objectivity in Social Science.** By Frank Cunningham. (*Toronto: University of Toronto Press,* 1973. Pp. 154. $8.50.)

This slim volume provides a convenient catalog of arguments and counterarguments invoked by those who deny and those who affirm the possibility of "objectivity" in social science. As such it should be helpful to those professional social science practitioners who have neither the time nor disposition to pursue all the arguments and counterarguments concerning this issue bruited by real or pretended philosophers of science. The book provides reasonably adequate accounts of the basic arguments construed as supporting some one or another form of "anti-objectivism" culled from the works of notables such as Thomas Kuhn, Benjamin Lee Whorf, Karl Mannheim, Norman Hanson, Peter Winch, Michael Polanyi, P. K. Feyeraband and Juergen Habermas. Professor Cunningham's counterarguments to "anti-objectivism" are themselves reasonably adequate, and generally follow the lines of argument articulated by specialists such as Israel Scheffler, Ernest Nagel, Richard Rudner, and Adolf Gruenbaum. The reader, as a consequence, has the opportunity of supplementing, in a substantial manner, his repertoire of cognitive responses to some of the more interesting questions that seem seriously to disturb the contemporary social science community.

Cunningham's book is a commendable attempt to support the claim that there are no defensible arguments for maintaining that the descriptions and explanations entertained by social scientists are somehow "nonobjective" functions of the social scientist's language, thoughts or desires regarding them. Cunningham insists that whatever we know of social science practice, social science products, and their underlying philosophy of science rationale, supports the claim that social science, does in fact, reveal something of the actual nature of its subject matter—and further, that there is, in principle, a public—that is to say, intersubjective—strategy for resolving the truth claims of rival empirical theories.

The book is effectively organized, and the principal arguments against scientific, and particularly social science, objectivity are collected in chapters devoted to arguments from (1) the nature and history of science; (2) linguistic relativism; (3) perceptual relativism; and (4) the special subject matter of social science. Arguments and counterarguments are presented in an intelligent and intelligible manner.

The major shortcoming of the book is that it is obliged by the very nature of the complex, often vague and ambiguous, character of the subject matter, to attempt too much. One can only accomplish just so much within one hundred twenty-three pages of text. As a consequence, there are any number of special, entailed, subsidiary, and tangential arguments that might have been pursued. The entire issue, for example, of the "objectivity" of the decision rules governing the acceptance or rejection of statistical hypotheses (particularly in social science research) is assessed and dismissed in about a page. On the other hand, we are sometimes teased with a suggestion that even moral issues might be "objectively" resolved (cf. p. 11)—but the suggestion is nowhere developed. But despite all such qualifiers, Cunningham's book, given its size, is admirable.

There is one curiosity about this book that requires comment: it is a book about the objectivity of science written by a convinced Marxist. To the perverse this might suggest something like a convinced Roman Catholic writing about the "objectivity" of the Church's strictures against birth control. To the less perverse it does suggest some speculation. Would Cunningham hold that the "theory" of Karl Marx is, in fact, supported by best evidence—that evidence that rests on discriminable and objective "fact," on rigorous classification and typology, on competent explanation, and reasonably precise and time-conditioned prediction? Or one might choose to speculate on whether Cunningham takes Lenin to be an "objective" social scientist and a competent philosopher of science. If such questions are not engaging enough, how about: Is Mao a Marxist social scientist? How about Fidel Castro? Or, perhaps to give the entire game away: Is Marxism the only objectively true social science? One day, Professor Cunningham will have to write a book about all that. For the time being it is enough that he wrote a good book about serious matters. That he chose to confound us by confessing something about his personal faith detracts not in the least from its merits. If he is serious about his methodological convictions—and I am sure that he is—he is the kind of "Marxist" the academic and scientific community could only welcome.

A. James Gregor

*University of California, Berkeley*

**Jean Bodin: Verhandlungen der Internationalen Bodin Tagung in München.** Edited by Horst Denzer. (Munich: Verlag C. H. Beck, 1973. Pp. xiv, 547. DM 78.)

Bodin is of course famous, and rightly so, as the chief developer of the theory of sovereignty in modern times; and it is among the merits of *Jean Bodin: Verhandlungen der Internationalen Bodin Tagung in München,* a multi-lingual collection of some two dozen essays on Bodin, that the reasons for this fame are impressively unfolded. One says "developer" and not "inventor" with good reason: the notion that *princeps legibus solutus est* was a key point in Roman law, as F. H. Hinsley has made plain in his fine *Sovereignty;* and Walter Ullmann has shown how much of the theory of sovereignty was (at least) incipient in some of the arguments against Boniface VIII's claims for papal universalism in his notorious *Unam Sanctum* in the early 14th century. But Bodin is usually taken to have been first to present a fully worked out and coherent sovereignty doctrine: he defined the sovereign power as that which can "give laws unto all and every one of its subjects and receive none from them," a power which is supreme, and not merely one among other law-giving public authorities (the Church, various medieval *collegia,* etc.). Bodin's sovereign was to be above the positive law (as its creator) but subject to divine and natural law; it was also to respect the fundamental constitutional laws of the realm and subjects' property rights. Bodin's sovereign was, in short, though the highest and indeed final authority, a *law-giving* authority, not an arbitrary or capricious one; the distinction between absolutism and arbitrariness was as important to Bodin as it was to be to Bossuet's *Politique tirée des Propres Paroles de l'Écriture Sainte* (a book which revolves around this very distinction). Bodin was first to conceive a nonarbitrary absolute political power in this sense; he not only developed the concept of internal sovereignty within a single state, however, but went on to deny the existence of a *Respublica Christiana* overarching the various states and to develop a new theory of international relations based on state autonomy rather than on "universal" institutions such as the papacy and the empire. Thus Bodin's theory of sovereignty simultaneously undermined the claims to political authority of all groups and institutions *below* the level of the national-state and the claims of all "universal" institutions *above* the state level.

It is true enough that Bodin did not have the "last word" on sovereignty and that later writers, particularly Hobbes and Rousseau, took the doctrine of sovereignty even farther than did he. As Robert Derathé shows, in one of the best of the essays in *Jean Bodin* ("Bodin dans l'Histoire des Théories de la Souverainété"), Rousseau dropped Bodin's notion that sovereigns must operate within the limits of "fundamental" laws when he urged that "it is against the nature of the body politic that the sovereign impose upon itself a law which it cannot infringe"—though here, of course, Rousseau is thinking of the sovereignty of a people whose general will is "always right," a concept quite foreign to Bodin. It is also true that a certain inflexibility in Bodin's theory of sovereignty led him to some odd political analyses: his insistence on the indivisibility of sovereignty made him insist that the 16th century Holy Roman Empire was an "aristocracy" because the seven electors of the empire chose and might depose the emperor, whereas the reality of the case was far more complicated, involving a mixture of declining feudal monarchy, rising regional states, "free cities" such as Hamburg and Lübeck, *inter alia.* Not even Bodin's distinction between "form of state" and "form of government" (well treated by Horst Denzer in another of the essays, "Bodins Staatsformenlehre") made his treatment of German politics persuasive. (Voltaire, for all his flippancy, was nearer the mark when he confined himself to saying that the Holy Roman Empire was neither holy nor Roman nor an empire.) And in the case of a federal polity such as Switzerland the inflexibility produced still worse effects: sovereignty-doctrines, which rely precisely on the notions of absoluteness and indivisibility, are hard-pressed to account for territorial divisions of power and for multiple political allegiances (hence Bodin's treatment of federal systems as "alliances" of "sovereign" powers).

Despite all this, Bodin's theory of sovereignty is still taken to be the first full and adequate statement and defense of the doctrine; and it is at least arguable that the difficulty which Bodin faced in trying to explain sixteenth-century German or Swiss politics in terms of indivisible sovereignty was an almost universal difficulty in his time—though his refusal to countenance any notion of a "mixed state," explained well in the present book by R. J. Schoeck's "Bodin's Opposition to the Mixed State and to Thomas More," did not help matters. One can further point out, as does Julian H. Franklin in one of the most informative essays in *Jean Bodin* ("Bodin and the End of Medieval Constitutionalism"), that the theory of sovereignty was not altogether accurate even as an account of French politics, since it succeeded in "proving" the existence of an absolute sovereignty in France "only by ignoring or distorting considerable evidence pointing to a tradition of consent" (p. 166)—a point, incidentally, made as early as 1677 by Leibniz in Chapter 11 of his *Caesarinus Fürstenerius*, which urged that in France "aid is not obtained from the clergy, which is the third part of the kingdom, by mandates given from the plenitude of power (as they say), but by demands, negotiations, and discussions."

It would be quite wrong, however, to suggest that *Jean Bodin* treats Bodin's theory of sovereignty at the expense of the rest of his thought; on the contrary, a generous number of pieces is devoted to less familiar Bodinian topics. Here one could mention a most persuasive piece, "Bodin and the Idea of Order," by W. H. Greenleaf, which draws illuminating parallels between Bodin's *Six Books of the Commonwealth* and his less well known *Method for the Easy Comprehension of History;* three substantial essays on Bodin as an historian and as a philosopher of history; and an intriguing contribution by Kenneth D. McRae entitled "Bodin and the Development of Empirical Political Science," which makes the plausible point that Bodin was not just describing sovereignty (to return to that apparently inescapable subject) but also promoting it. There is even a piece by Christopher R. Baxter, "Jean Bodin's Daemon and his Conversion to Judaism," which will present to those familiar with Bodin simply as a theorist of sovereignty a completely "new" side of his character and writings.

If, though, one had to single out one of these essays as most helpful and instructive, one's choice might well fall on Raymond Polin's "L'Idée de Republique selon Bodin"—a piece which, while laying sufficient stress on the distinction between absolutism and arbitrariness, and on the notion that a Bodinian sovereign is hedged round by natural law, constitutional law and private property-rights, dwells at the end, as most of the other pieces do not, on what sovereignty is *worth.* And in this assessment Polin urges that while it is true that Bodin spoke often in the language of "necessity," and talked of a political power which was to be "absolute, infinite, perpetual," he was concerned that a sovereign "have as much obligation as power" and that his power be "in truth, a function of his obligation" to create and uphold laws which imitate divine laws (p. 356); that a state be possessed not just of absolute power but of "a rightful government, well-founded and assured public authority, harmoniously realized justice" (ibid.). Of the two dozen essays in *Jean Bodin* it is arguable that Polin's finds the most adequate equilibrium among all of the elements that Bodin valued, and puts the question of sovereignty's value above the question of its historical origins—though it does not go so far as to ask, with Laski, whether the notion of sovereignty has not lost whatever utility it might once have had; nor to ask, with de Jouvenel, whether the concept ever had enough validity to offset the danger involved in elevating a theory of "absolute, infinite, perpetual" power to a supreme principle of political right.

It remains to add that the editor, Horst Denzer, has appended to these essays a remarkably comprehensive bibliography and an excellent index, and that a 72-page "discussion" between the authors of the twenty-four essays has been appended at the end. Since this discussion proceeds in three languages (German, French, English), and sometimes in a mixture of several of them—Derathé, for example, sometimes answers his German colleagues' arguments in German, but makes his own observations in French—it will obviously be of use mainly to those able to switch languages rapidly. But it ought not to be thought that this book is closed to those who are not linguists; there are quite enough good pieces in English (nine out of twenty-four) to warrant the attention of readers who know English only. This collection, in short, merits careful consideration, and deserves the praise of those interested in Bodin's political thought.

PATRICK RILEY

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**Strategic Interaction.** By Erving Goffman. (Philadelphia: University of Pennsylvania Press, 1969. Pp. 145. $2.95.)

Erving Goffman has already demonstrated his ability to write interestingly on rational gamesmanship (see especially *The Presentation of Self in Everyday Life*), and this book will not disappoint his readers. *Strategic Interaction* amounts to two long papers, the first one, "Expression Games," and the second bearing the title of the book. While certainly linked, the two seem most probably to have been written separately and simply bound together in this volume. No matter, for Professor Goffman has stimulating points to make.

"Expression Games" is a fascinatingly introspective analysis of the kinds of ploys we may all use in "informing" others the way we want them informed. How someone else will behave is a function of what he believes about his opportunities, and about our intentions and character. Goffman shows how we try to alter such a person's beliefs to our benefit, how he will in turn cross-examine the evidence we thrust at him, and how we develop a powerful intuition about ways to get believed despite this cross-examination. Getting believed is no mean trick even when one is telling the truth, but Goffman depicts us all as being more expert than novice at it. The author, to prove his point, perhaps relies too much on examples drawn from "gee whiz" books about World War II subterfuge and espionage, some of which might be taken with a grain of salt. Yet his basic point is so clearly correct for most human interactions that he hardly needs the supporting or illustrative "big war" examples. The propaganda expertise we live by may thus rest at the level of everyday intuition. Goffman's forte is analyzing this in explicit and thoughtful detail. His analysis parallels some of Robert Jervis's writings (especially *The Logic of Images in International Relations*) (Princeton, New Jersey: Princeton University Press, 1970) and Thomas Schelling's *Strategy of Conflict* (Cambridge, Massachusetts: Harvard University Press, 1960).

The author's style is all the more interesting and remarkable because he is an anthropologist and sociologist by training, rather than an economist or political scientist. Sociologists have usually tended to stress the inadvertent over the premeditated. In Goffman's uncovery of and emphasis on conscious behavior and rational goal-serving mechanisms, there is thus an enthusiasm and excitement which an economist (for whom all of this is old hat with a suspicion of tautology) more typically might not display.

The second paper on "Strategic Interaction" amounts to an interesting step-by-step explication of strategic analysis in social situations as intuited in ordinary English. Can one demonstrate by any stream of consciousness that we rationally pursue explicit goals in such situations? Goffman's essay spins out a persuasive illustration that we indeed do. One cannot claim that Goffman's structures and definitions here are thoroughly new or revolutionary, although some of his asides on the inputs of sociological analysis for the sorting out of game-theoretical problem types are important. The essay is valuable because it is so thoroughly readable, and profoundly independent, the product of someone working through the conceptual forest again, to find very parallel trails.

Goffman throughout relies heavily on his intuition, illustrated by examples taken from a broad acquaintance with writings in history and politics, but never worrying about whether his $N$ is large enough to generate some sort of statistical reliability. His problem, as with any other believer in strategy, is almost a form of Heisenberg uncertainty principle: since it is obviously so sensible for actors to pretend to be less clever and devious than they really are, we must assume that they are more clever and devious than they seem. Where the evidence does not leak out, assume it, or get it by introspection, for the proof of strategy is that one does not indeed see all the strategy.

We may no longer have any shortage of lively books on strategic reasoning. Yet anyone who felt ill-equipped, for purposes of teaching students, or prodding his own research imagination, would do well to read this book.

GEORGE H. QUESTER

*Cornell University*

**Social Justice and the City.** By David Harvey. (Baltimore, Md. The Johns Hopkins University Press, 1973. Pp. 336. $10.50.)

"Cities . . . are founded upon the exploitation of the many by the few" (p. 314). Thus David Harvey concludes in this polemical book. He observes that a "genuinely humanizing urbanism has yet to be brought into being" (*Ibid.*). The first half of the book considers liberal theories of urbanism; the second half formulates a revolutionary theory.

First we are told of the difficulties urban specialists have in relating decisions affecting the spatial form of the city (where will housing or means of transportation be located?) with decisions affecting the social processes going on within the city (employment, welfare services, etc.) so as to accomplish some "coherent social objective" (p. 50). Both kinds of decision actually redistribute income, defined as "all receipts which increase an individual's command over the use of a society's scarce [man-made as well as natural] resources" (p. 53). So both may be linked by a conception of social justice defined as an equitable distribution of income. Presently decisions allocating urban space and governing urban social processes tend to cumulate income advantages for the rich. For example, improvements in public transportation enable the affluent to move swiftly from downtown to the suburbs. Since the employment opportunities in the suburbs are growing, the impact on income distribution of the decision to build public transportation is regressive; the black domestic cannot even find a bus in her neighborhood to take her out to the suburbs. Worse still, efforts to counteract income inequities seem futile. For example, the effort to attract investment funds to inner-city rental housing will cause a shortage of investment funds for suburban home construction; since the suburbs can afford to do so, they will increase interest rates and attract these funds back to the suburbs.

The fault of all liberal theories of urbanism is that they deal with symptoms; they do not deal with underlying causes. "Programmes which seek to alter distribution without altering the capitalist market structure within which income and wealth are generated and distributed, are doomed to failure" (p. 110). Thus liberal theories of urban phenomena amount to an apologetic for the existing inequitable income distribution (p. 147). For instance, present day urban land use theory separates the value to the occupier—the use value—from the exchange value of land and property. A socialist theory is competent to explain why the use value of land or property, which should be the primary consideration from the point of view of social justice, is not. The market economy presupposes maintenance of an artificially created scarcity of land and property. As long as there is scarcity, exchange value will predominate over use value. Maximization of exchange value by some groups automatically reaps disproportionate benefits for the few (p. 175). Social justice is unattainable for as long as the mechanism responsible for these inequities—the free market system—is preserved. ["The rentier will get that pound of flesh no matter what" (p. 191).]

The city itself, in its totality, is merely a convenience for the market system. The consumers conveniently collected together in the city have needs which can be easily manipulated; they consume the surplus product of capitalist society and by doing so preserve the artificial scarcity requisite for high profits for a few. The city serves as a repository for the surplus value of a capitalist society (p. 231). Herein lies the flaw in the free market system: "if the rate at which surplus value is being appropriated at the centre [of the city] . . . exceeds the rate at which social product is being created, then financial and economic collapse is inevitable" (p. 264). Nor will efforts to restrain the monopolistic practices succeed; such efforts will simply hasten the inevitable collapse. "A more open and individualistic capitalism . . . would almost certainly destroy all civilized society" (p. 273).

Only the socialist view which recognizes that social injustice in the city is the inevitable consequence of inherent flaws in the free market system can inspire a theory which is not apologetic for the *status quo*—a revolutionary theory. And with such a theory the revolutionary practice needed for a "genuinely humanizing urbanism" (p. 314) may be achieved. "The Chinese and Cuban efforts to promote growth with social justice are probably the most significant so far undertaken" (p. 115).

In Harvey's book, the free market system assumes a life of its own independent of politics; the political process merely reflects the dominant economic forces (p. 73). Existing inequities are held to be due to the operation of an economic mechanism rather than to defective political arrangements. Harvey ignores the connection between racial tensions and urban problems. Also, he predicts, without any supporting evidence, that labor will be driven to subminimum subsistence levels. (p. 224).

The critical step in Harvey's argument is his definition of income as command over all resources, manmade as well as natural. Once income is defined so roomily, it is easy to view all decisions in the city as primarily economic. Since there does not seem to be any conspiracy orchestrating the growing income inequity, it makes sense to attribute the cause to an underlying economic mechanism—the free market system. This argument might be credible if and only if Harvey demonstrated that the "income" inequities he inveighs against are not the result of a web of conflicting if not coordinated political judgments. Also, his argument would have been strengthened had he defined his view of social justice, instead of asserting somewhat mysteriously that a valid conception of social justice can be verified only in practice (p. 12).

A happier scheme of organization might have been chosen—some scheme other than forcing the reader to struggle along the same paths Harvey apparently followed in evolving his "revolutionary theory." A coherent statement of his thoughts in their final version supported with an array of reasons and evidence would have made the book more forceful and more intelligible.

The book contains several compelling insights, yet the nagging doubt remains that it reveals more about Harvey's theorizing than about socialist theorizing about urbanism.

PHILIP B. LYONS

*House Budget Committee*

**Tools for Conviviality.** By Ivan Illich. (New York: Harper and Row, 1973. Pp. 110. $5.95.)

Illich, in this his latest book, identifies a nearly idyllic future state. This state, the convivial society, is defined as ". . . a society in which modern technologies serve politically interrelated individuals rather than managers" (p. xxiv). The convivial society entails ". . . autonomous and creative intercourse among persons, and the intercourse of persons with this environment; . . . individual freedom realized in personal independence" (p. 11).

The author is closely identified with the Centro de Información y Documentación (CIDOC) located in Cuernavaca, Mexico. The publications of those associated with CIDOC, including Illich's *Deschooling Society,* are critical of the structures and institutions of advanced capitalist societies. The thesis of *Tools of Conviviality* can be stated simply: Technology, originally a servant of man, is now more and more man's master. Illich finds this technologically conditioned lifestyle basically anticonvivial and hostile to the full development of human potentials. The United States is the prime example of an anticonvivial society.

To make his point about the increasing abuse of technology, Illich turns to the field of medicine. In an oversimplified analysis, he finds that doctors early in this century focused much of their energy on improvement of the quality of life through the use of a limited number of medicines and by working for water purification, personal hygiene, and control of rodent-transmitted diseases. Since 1955, he holds, the doctor-patient relationship has become increasingly impersonal; doctors spend increasing amounts of time treating doctor-induced diseases and in generating demand for additional medical services. Medicine increasingly has become man's master rather than man's servant.

This evolution from servant to master can also be seen in "education, the mails, social work, transportation, and even civil engineering . . ." (p. 26). Regrettably, Illich does not attempt an analysis of developments in these fields.

Having made the point that technology (tools) can become more master than servant of men, Illich proposes to "lay down criteria by which the manipulation of people for the sake of their tools can be immediately recognized, and thus to exclude those artifacts and institutions which inevitably extinguish a convivial lifestyle" (p. 14). While his discussion of the criteria is confused and rambling, he does include networks of multi-lane highways, strip mines, and compulsory school systems as tools which "inevitably increase regimentation, dependence, exploitation, and rob not only the rich but also the poor of conviviality" (p. 26).

Convinced of the validity of his analysis, Illich believes that the possibility of a convivial society can be transformed into a political program if it can be shown that "the structure of our tools menaces the survival of mankind . . . and that this danger is imminent" (p. 45). Rather than show that the apocalypse is near, Illich presents a well-thought-out indictment of the evils of industrial development too detailed to summarize here.

The apocalypse, according to Illich, can be avoided only by a bureaucratic dictatorship or by engaging "in a political process by the use of legal and political procedures" (p. 100). The latter alternative becomes viable only in the context of what Illich calls a "multiple catastrophe," which he defines as a "crisis in the industrial mode of production" (p. 107). In the face of this catastrophe, a nascent and growing coalition advocating a convivial lifestyle would emerge as a majority, thus obtaining control of legal and political structures.

Illich's politics are as utopian as his vision of a convivial society. "No present government could restructure society along convivial lines" (p. 16). Although Mao's China receives favorable mention, the author contends that "different balances . . . in the distribution of goods and services can prevail in societies striving for conviviality" (p. 24). His notion of an impending multiple catastrophe in industrial societies is not convincing. *Tools of Conviviality* does, however, provide a brief and insightful critique of the problems of overindustrialization. Would that Illich had written more on that subject!

GAYLE AVANT

*Baylor University*

**Guillotine in the Wings: A New Look at the French Revolution and Its Relevance to America Today.** By Alex Karmel. (New York: McGraw-Hill, 1972. Pp. 237. $6.95.)

It is difficult to tell whether this book is harder on the French Revolution or on "America today." In any case, it is a literary tribute to the banality of relevance. Karmel has written his account for those who "tend to think of the French Revolution as a bolt of lightning across the sky of history" (p. 3) and for those who believe it was "all blood and gore and heads popping into baskets" (p. 4). Having made clear that bolts of lightning and heads popping were the least of it, and having set the record straight on certain other crucial issues (e.g., "everyone gets older every year . . ." and "one nice thing about time, it moves only forward") (p. 15) of more concern to historians than social scientists no doubt, he gets down to the real business at hand: a "new look" at the "original granddaddy of all revolutions" (p. ix).

Presumably because of his commitment to relevance, Karmel has dispensed with the usual paraphernalia of scholarship—footnotes, references, bibliography—leaving the reader to trust in his rather unique credentials (several novels and a "fictional autobiography of Restif de la Bretonne." Relevance supplies what scholarship has denied: "Joe McCarthy," for example, turns out to be "the portrait of a twentieth century Robespierre" (p. 69); Marat, on the other hand, "would have understood and applauded the prison revolt at Attica" (p. 115). Louis himself, for all "his good intentions . . . made a mess of everything" (p. 66)—an assertion which, I must admit, hardly seems to require documentation. Not that Karmel's account is exclusively *ad hominem.* Relevant ideas also receive attention. We learn, for example, that the idea of Rousseau's General Will was "one of those abstract ideas tucked away in philosophical works that can be dynamite with a long fuse" (p. 105). Indeed (shades of J. L. Talmon?), "the whole mentality of the modern totalitarian state was created overnight" (p. 161). Of course it took some time for this to be recognized because "there was no six o'clock news in Paris in 1793." This latter datum, though it will come as a revelation only to the "bolt of lightning" crowd, is a useful contextualist reminder to fans of Lovejoy who are forever reading universal themes into particular times and events. At least I think it is.

Toward the end of his remarkable rampage through history, Karmel confesses that he has "been led to use the kind of language that belongs more properly to fiction," presumably to explain such "language" as this: "even the most kinky forms of sex antedate Neil Armstrong's setting foot on the chaste bosom of Diana" (p. 227). (Relevance accounts for the subject, my point is the stvle). But Karmel does not do himself justice. For *Guillotine in the Wings* is not to be judged by the standards of historical fiction, much less the quaint requisites of serious scholarship. Its unusual virtues belong to an entirely different genre whose parameters are defined by Metro-Goldwyn-Mayer historical spectaculars and the modern pulp novel. *The American Political Science Review* ought perhaps in the future to assign such works to Rex Reed for review. Or Spiro Agnew.

BENJAMIN R. BARBER
*Rutgers University*

**The Great Jackass Fallacy.** By Harry Levinson. (Boston, Mass.: Harvard University Press, 1973. Pp. 178. $7.00.)

The Thomas Henry Carroll-Ford Foundation Distinguished Visiting Professorship of Business Administration at Harvard University was held by Harry Levinson from 1968–1972. During this period, Professor Levinson produced three books, in addition to the one being reviewed. Books that have artful titles, and that represent an integration of previously published papers, are often less than able contributions to knowledge. This book is a happy exception to that rule.

Professor Levinson has made a significant effort in this book to present sound conceptual designs which add to the ever-growing literature in organization theory and behavior. Much of the book is based upon past articles published in the *Harvard Business Review* and in other journals. One new chapter (chapter 11) has been added.

From his long experience in organizational psychology, Levinson suggests that the dominant philosophy of motivation in American management is that of reward and punishment. In effect, this is an image of a carrot at one end and a stick at the other. The central figure of that image is a jackass to be manipulated and controlled. One may assume that in many instances university administrators and leaders of public and private bureaucracies hold this view. Organizational administrators assume that they are dealing with jackasses who are stubborn, stupid, and willful. Those who exercise leadership in organizations, and who operate within reward-punishment assumptions, imply that they have control over others, and that these individuals are in a jackass position in their relationships to them.

Levinson presents his own theoretical framework within which alternatives to the jackass fallacy can produce effective organizational effort. His main theoretical assumption is that of psychological man, which is based upon the product of four interacting forces (1) psychological motivation, (2) social role encompassed in the concept of work, (3) a modal organization structure within which people work, and (4) a broader ethos within which people function.

This book makes several worthwhile theoretical contributions to current organizational analysis. First, several parts of it deal with the implications of the emotional toxicity of work and of the responses of organization members to middle age and to loss. Given the fascination with change, planned or otherwise, which tends to dominate the present state of organizational knowledge, this book clearly argues that all change is a form of loss. Loss engenders important psychological and physiological consequences. Persons who suffer loss tend to recoup their losses and compensate for them. Second, two important recent theoretical approaches to organizational theory, Management By Objectives (MBO) and Organizational Development (OD), are systematically analyzed by Levinson. Many analysts have already raised the issue about whose objectives are determinants of managerial philosophies as posited by MBO. We are aware that responsible practitioners of MBO are recasting its original assumptions. Certainly the objectives of an organization must be formulated by those at varied levels of the hierarchy, and not only by those at the top. In Levinson's view, MBO misses the whole human point. He suggests that OD often fails to engage in genuine organizational diagnosis. Its techniques are too often *ad hoc* and are based upon expedient assumptions. A diagnosis would require a comprehensive examination of the client's system. Interestingly, Levinson asserts that there is no systematic body of knowledge about organizational development. Recent volumes on OD have spent too much time trying to answer the question of whether or not OD is merely a fad. Third, Levinson deals perceptively with the problem of revitalizing a bureaucracy, with special focus upon the Department of State. Building upon the work of Argyris and others, he makes several pertinent points: (1) Bureaucracies cannot be changed from the outside, and (2) if bureaucracies cannot be changed by the top, by the inside or by the outside, the only conditions under which they can be changed is when there is pressure from all three. A mechanism of simultaneous task forces will bring the organization face to face with its realities. These realities would include the degree of support required from the consultant, the level of competence and skill to effect change, and the capacity to master problems.

This volume can be compared to the work of Chester Barnard in terms of its wisdom and ultimate contribution to organizational theory. It is to be hoped that those who did not read its parts when they were originally published will avail themselves of the materials contained in this book.

EDWARD R. PADGETT
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**Il Liberalismo in un Mondo in Trasformazione.** By Nicola Matteucci. (Bologna: Società Editrice Il Mulino, 1972. Pp. 266. Lire 3,000.)

What is liberalism? Will any two scholars ever agree on an answer? Probably, if we are flexible enough in our characterization of this important movement. We American political scientists generally know what we mean by liberalism (with a small "l"), assuming that we have a modicum of familiarity with the history of political thought (admittedly a generous assumption in some quarters). We will have read Frederick Watkins, Guido de Ruggiero, and John Hallowell, and from them and others we will know that, at its best, liberalism is that broad political outlook, commencing roughly with Grotius and Locke, and going through various transformations in response to different challenges, which welcomes the secularization of political life, individualism, consent, peaceful change, limited government, and moderation in all things.

Professor Matteucci, who holds the chair in the History of Political Doctrines at the University of Bologna, is well qualified to reassess the condition of liberalism in the light of its contemporary situation. He has written on de Tocqueville, McIlwain, and constitutionalism and possesses an excellent grasp of the literature on liberalism, both European and American. He has drawn inspiration from one of the greatest liberal minds of this century, Benedetto Croce.

Matteucci's book is felicitously written; its argument is urbane and undoctrinaire. Nonetheless, for all its merits, the volume fails to give a sustained treatment of a few major themes. Instead, it flits briefly over a number of topics, both historical and contemporary, leaving the impression that the book lacks sufficient unity and depth to be accounted a major contribution.

One of the interesting subjects to which the author does devote more space than he does to others is the "challenge" which industrial or technological society holds for contemporary liberalism. Matteucci is eloquent in his advocacy of a "multi-dimensional society" over against the "one-dimensional society" of the messianic technocrats. He accurately assesses the ideas of St.-Simon, Comte, and their contemporary apostles, however, beneficent in intention, to be profoundly illiberal in their premises and in the results of those premises in the society in which we live.

Matteucci is right: everything considered, liberalism is the most decent, humane, adequate, and tolerant of the various political doctrines to appear in the modern period. It has endured for at least three hundred years and, as he suggests on pp. 73–74, has resisted, with considerable success, three great "challenges": those of political absolutism (in the eighteenth century), the "social question" (in the nineteenth century), and totalitarianism (in the first half of our own century). But will present-day liberalism be adequate to meet the challenge of "postindustrial" society, and the threat of the technocratic ethos to mobilize all desires, structures, and symbols in the name of the latest idols: productive efficiency and technical rationality?

These considerations are practical in nature, and they raise the question of whether liberalism is a *doctrine* responding to the exigencies of the political struggle or a *philosophy* capable of transcending the immediate priorities of that struggle in order to consider the perennial features of the human condition. If liberalism is primarily a doctrine responding to immediate crises, then its conclusions, however effective practically, must theoretically have only partial validity, because they are inevitably *responses* to those immediate challenges and are conditioned by them. To understand why conservatives and radicals, for example, in vastly different ways find the liberal doctrine of man and society deficient would require a noetically critical perspective, which is to say a perspective different from that of even a most intelligent liberal publicist such as Professor Matteucci.

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**Imperialism and Revolution: An Essay for Radicals.** By Bernard S. Morris. (Bloomington: Indiana University Press, 1973. Pp. 81. $5.00, cloth; $1.95, paper.)

This is another product of the Vietnam war, an echo of the passionate debates and protests which rocked the campuses only a few years ago but now seem to belong to another age. Professor Morris writes as though he were speaking to a group of indignant students, accepting as much of their case as possible and using some of their language while gently explaining that the Leninist theory of imperialism as expansion dictated by monopoly capitalism really won't wash in the case of Vietnam. The refutation is mild; Lenin's political interpretation, which in an exact science would hardly merit the designation of "theory," is left seeming less logically deficient than it really is. The neo-Marxist theory of imperialism, he avers, is "the best we have" (p. 26). The author then proceeds to take apart George Liska's 1967, and now outdated, rationalization of the American role as world keeper of law and order. Morris, like the protesters, was profoundly moved by the wrongness of the American actions in Vietnam, impatient with the militarization of United States foreign policy, and doubtful of improvement without fundamental change in American society. He gloomily felt that the public had been so brainwashed that "change indeed seems almost impossible" (p. 57), and that "the governing class has become so powerful as to cast doubts on the viability of the democratic process . . ." (p. 35) Revolution is hence the secondary theme of the book. Unhappily, he does not find in neo-Marxism or anywhere else a real promise of radical change; and he concludes with a wan hope of a new consciousness, à la Charles Reich.

This is all well written and generally well pondered, and the points made are mostly unexceptionable. But it does not tell us much. It is dubious reification to treat "imperialism" as a coherent, analyzable phenomenon. Many reprehensible actions of stronger states in relation to weaker ones may be called "imperialistic," but it is not clear that they necessarily have much in common. Professor Morris concedes (p. 50) that Roman imperialism, which had nothing to do with capitalism, was a very different breed from what he sees as American imperialism; but he does not discuss whether the colony grabbing of the late nineteenth century was really of a kind, in motives and methods, with the American role in Southeast Asia or the "neo-colonialism" through which Third World economies are dominated by foreign, largely American corporations without specific action by the American government. He notes (p. 39) David Landes's definition of imperialism as "a multifarious response to a common opportunity that consists simply in a disparity of power." But this is so broad as to be uninteresting; and it allows any action of the stronger power to be called imperialistic.

The whole discussion, in any case, has become passé with extraordinary rapidity as the U.S., chastened by the Vietnam nonsuccess, pulls inward. If this nation spent the past generation, during which it possessed always overwhelming economic power and usually overwhelming military potential, in empire-building, there is little to show for it, politically or economically. Almost powerless Arab countries have demonstrated how they can not only manhandle the supposedly dominant giant corporations but send a chill through the economy of the whole Western world; and the mighty imperialists hesitate even to mutter about countermeasures. No one can feel certain that premodern ways of thinking may not lure policy makers one day to new interventions, but the problems to which Professor Morris addresses himself no longer seem to be the great issues of the day.

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**Modern Organizational Theory: Contextual, Environmental, and Socio-Cultural Variables.** Edited by Anant R. Negandhi. (Kent, Ohio: The Kent State University Press, 1973. Pp. 404. $10.00.)

The twenty-two papers which comprise the present volume grew out of a series of four annual conferences held by the Comparative Administration Research Institute at Kent State University between 1968 and 1971. At these meetings scholars in the field of organizational theory presented a wide variety of viewpoints in papers and discussions examining the impact of size, technology, environmental, and sociocultural variables on organizational structure, behavior patterns, and effectiveness. Selected and edited in light of the discussion they generated, these papers provide a valuable survey, through both theoretical and empirical studies, of the currently developing research strategies and theories of organization.

The volume is divided into four parts. Each part comprises a number of original papers as well as a number of critical appraisals.

Part I is entitled "Contextual Variables: Size, Technology, Ownership, and Dependence." Here we find papers by Arlyn J. Melcher, Derek Pugh and Diana Pheysey, Charles Perrow, and Joan Woodward.

The first two papers are focussed generally on the impact of size, hierarchical structure, spatial-physical-temporal factors, leadership style, information flow, and control mechanisms upon organizational behavior and effectiveness. The latter two are concerned primarily with a definition of the concept of technology and the specific impact of technological factors on organizational functioning.

Part II is entitled "Environmental Variables and Interorganizational Focus." Included here are papers by Louis C. Gawthrop, Basil S. Georgopoulus, Jay W. Lorsch, Roland L. Warren, et al., Mark Lefton, together with a paper jointly authored by Paul E. White, Sol Levine, and George J. Vlasak.

The first two papers, by Gawthrop and Georgopoulus, explore the implications of major social and technical changes, both current and prospective, on governmental bureaucracy and hospitals, respectively. The next paper, by Lorsch, explores the effect of differing market and technological conditions on organizational structures and effectiveness. The fourth paper, by Warren and colleagues, explores the complex pattern of relationships between public school administration offices, health and welfare planning councils, community action agencies, as well as other so-called "community decision organizations." Lefton's essay explores the consequences for organizational behavior arising out of the specific interests which given organizations have in their client-organizations. The final essay, by White, Levine, and Vlasak, elaborates upon the utility of "exchange theory" for understanding interaction among social organizations.

Part III appears under the heading: "Socio-Cultural Variables." It includes papers by Michel Crozier, Pjotr Hesseling, Richard N. Farmer, and, not least, a paper by Anant R. Negandhi, the editor of the present volume.

The paper by Crozier depicts the manner in which the power relationship in organizational settings is influenced by the sociocultural characteristics of the people involved in such relationships. The second paper, by Hesseling, reports on a multi-national study designed to cast new light upon the impact of four independent variables, namely, ecology, culture, technology, and the power structure, on organizational behavior and effectiveness. The essay by Farmer presents a classification schema by means of which various sociocultural, economic, political, legal, and educational factors influence organizational practice and effectiveness. And while this paper is cast on a macro analytical level, the last paper in this section, by Negandhi, attempts an alternative micro level conceptual scheme with which to examine the impact of environmental, sociocultural, and organizational variables on management practices and effectiveness.

The fourth and final part is presented under the rubric: "Introducing Change in Complex Organizations." Here we find papers by Gene W. Dalton, and Berton H. Kaplan, respectively. The former paper by Dalton marshalls the available evidence showing that significant alterations of individual behavior patterns are possible. The essay by Kaplan explores the problem of how the traditional bureaucratic model will have to be modified in order to adapt to the changes taking place in modern society.

Included also in this section is a short epilogue entitled "Beyond the Present Trends," by the editor, Negandhi, in which he shares his critical concerns for the future direction of organizational research. Negandhi deplores the prevailing emphasis upon structure at the expense of function, the apparent lack of concern about the linkages between important variables, and the general lack of interest in the processes by means of which organizations adapt and cope with environmental forces.

With all of this one can concur most heartily. For it appears that organizational theorists, as here represented, regrettably share with their sociological compeers a decided preference for methodological viability at the expense of theoretical adequacy, and for correlational rather than causal study designs. The reader will search in vain for any evidence of movement toward a processual approach which recognizes that any given item of behavior may very well have both multiple causes and multiple consequences.

But despite the numerous shortcomings and limitations of the present collection of organizational studies, there is some warrant for both gratitude and optimism. For it does appear that the crucial image of an organization situated in an environment of other organizations to which adaptations must be made has at long last been permanently established in both organizational and administrative theory. This notion, having been forcefully and dramatically brought to stage-center, will hopefully inform and illumine the formulation of future research studies in ways that more effectively meet the criticisms of prevailing research designs and approaches. And to the extent that this constitutes a realistic expectation, it seems reasonable to suppose that political science in some or all of its specialized focii of inquiry, can look forward to an increasingly more effective rapprochement with a sociology of organization and administration.

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**The Concept of Representation.** By Hanna Fenichel Pitkin. (Berkeley: University of California Press, 1972. Pp. 323. $8.50, cloth; $3.65, paper.)

Hanna Pitkin's volume on the concept of representation is the crowning achievement of many years of hard effort—a fine example of Hegel's *Anstrengung des Begriffs*. It encompasses the work of many others, and does so ably and fairly. It promises to become a classic in this central and important concept of political theory and related fields. Its writing is condensed and the presentation of difficult issues clear. A masterly treatment of which I can say that I would be proud to have done it. Not that I agree with all Professor Pitkin says; far from it.

Pitkin starts with a careful presentation and analysis of Hobbes's handling of representation. She designates it as the authorization approach, which starts from the individual but leaves Hobbes' individualism unanalyzed. I regret this, because it leaves the problems which she rightly points out as shortcomings of Hobbes's approach without solution. Hence, the reader is not told clearly that Hobbes is wrong. What was needed here was a deeper inquiry into the phenomenon of authority—political authority as rational elaboration. [For a more detailed statement of what is involved, see my *Man and His Government* which appeared in 1963 and might have helped Professor Pitkin in strengthening and clarifying her position.] Unfortunately, like so many political scientists, she uses the term "authority" uncritically, and as it is customary in the legal tradition [see my *Tradition and Authority*]. I find myself handicapped here by my own work which laid a firmer foundation for representation than did the authors with whom she is primarily concerned. She justifiably defends herself against the accusation of being preoccupied with words, but tends to fall into the related error of being preoccupied with *one* word without regard to the family to which it belongs.

Her neat tracing of representation to its classical origins might usefully have been implemented by some attention to the question of why the Greek and Roman thinkers when confronted by the fact of representation did not explore its nature. The oft-remarked failure of the Greeks in the field of federalism is directly related to their lack of insight into the potentialities of representation. (Rome's failure is less often commented upon, but even more glaring and disastrous in accepting empire as the only solution of the problems which territorial expansion created.)

Professor Pitkin is laudably explicit in her critical comments upon the popular misuse of terms like representative government. Such terms have been important weapons in the Anglo-American armory of propagandistic manipulation of analytic terms. (Another striking example of this propensity is the talk about "rigid" versus flexible constitutions in adulation of parliamentary representation.) She rightly insists that in one perspective, every government is representative; she might have added that in another, none are. Yet, realistic political analysis has to face the question of more or less. Is Nixon (or any other public official) always representative to the same degree? If not, what explains the change? What, in other words, constitutes the representativeness of representatives? Is there some empirical ground for calling a proportional representation scheme more representative than another? Pitkin offers sane and insightful comments on these matters so hotly disputed and so fanatically proclaimed over the years. Here again I regret her failure to penetrate deeper than her single focus; for since the argument is conventionally cast in terms of "justice," what makes one representation more just than another or indeed *more* anything? There is a lack here of sensitivity to the *quantitative* dimension of representation. I am no fanatic of the quantitative approach, to be sure; but I do maintain that it is a significant perspective yielding new insights. This is as true for representation as for other basic issues of politics. An interesting practical illustration is found in federal orders. Is it vital that the component units are represented according to their size, or may they have equality regardless of their size? Involved here is the familiar issue of one vote for each person. The U.S. is not the only federal system which for expediency's sake has been run on the basis of disregard for such numerical equality, and will continue to do so, despite the Supreme Court's preference for the "one man, one vote" principle.

In addition to its other great merits the book is well printed and provided with a good index and a bibliography. The latter is by no means complete; I missed some authors who have devoted much labor to the study of representation: Dolf Sternberger, formerly professor at Heidelberg; and Gerhard Leibholz, former federal judge at Karlsruhe and Professor at Goettingen. East European authors are almost entirely missing. It is a minor blemish in a work so thorough in its learning and so painstaking in its scholarship. I wish that Professor Pitkin might find time to explore the concrete experience of federal systems, past and evolving. More especially the European Common Market needs her analysis, and so does Federal Germany. And what about our own Puerto Rico? Is Puerto Rico properly represented as an associated free state? What about her resident commissioner? Popular election of this vital representative is supposed to solve the problems of Puerto Rico's representation. Does it? Finally what about the UN? Are the US and Nicaragua equal? And if not, how can they be properly represented?

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**Sex-Pol: Essays, 1929-1934.** By Wilhelm Reich. Edited by Lee Baxandall. (New York: Random House, 1966, 1971, 1972. Pp. 378. $10.00.)

Political scientists, if they have heard of Reich at all, mistakenly associate him with sexual libertinism, or the notorious orgone therapy, for the promotion of which through the mails Reich was sentenced to a term in the Lewisburg federal penitentiary (where he died in 1957 at the age of sixty). If one regularly sat in a so-called orgone box, Reich believed, one could recapture the vital life energy that had been lost, and by that means regain health and productivity. The orgone itself, in effect, not to be confused with what Freud meant by *libido,* was orgastic potency in quantifiable form; unlike most of the early psychoanalysts, Reich recognized that ejaculation and orgastic potency are not identical. By now this finding is broadly accepted, but where Reich went wrong was to assume that orgastic energy was, like electricity, something that constantly was being released into the atmosphere and therefore could be recaptured. The orgone box allegedly trapped this energy before it could be further depleted, and returned it to the body.

It is of interest that this Reichian excursion into input-output analysis, if I may use that overworked expression, with reference to the psychobiophysiology of sex nowhere is mentioned by *Sex-Pol* editor Lee Baxandall or by Professor Bertell Ollman (who wrote an introduction). Nor do we learn anything about Reich's significant contributions to psychoanalytic theory or his repudiation toward the end of his life of much of what he had earlier believed about the relationship between sex and politics. Indeed the few references to Reich's troubled memberships in the German Communist Party (which ended with his expulsion in 1933), and in the International Psychoanalytic Association (from which he was expelled in 1934), are incorrect or misleading. Reich was in trouble with some of the leading figures in the IPA, although not apparently with Freud himself, long before he published *The Mass Psychology of Fascism* in 1933, and it is irresponsible if not dishonest of Professor Ollman to imply that his expulsion owed something, "as difficult as it is to believe today," to the IPA's hoping "to make their peace with fascism." No doubt some of the German members of the Association wished to avoid difficulties, but such wishes were doomed from the beginning even apart from the fact that Jews were prominent in psychoanalytic circles. Between psychoanalysis, which can not be understood except in the context of the Judaic-Christian tradition, and the paganism and nihilism of Naziism, or looked at another way, between the eros of Freud and the thanatos of Hitler, there could be no common ground, and with the exception of Jung, who cooperated with Goering until 1940, no prominent psychoanalyst went over to the Nazis.

Baxandall, of course, is much less interested in presenting Reich's credentials as either a Marxist or a psychoanalyst than in establishing him as the major theorist of sexual politics. "The Imposition of Sexual Morality" stresses the connection between capitalism and sexual repression and because this assumed relationship is crucial in the thinking of such apostles of revolution and/or counterculture as Roszak, Slater, Charles Reich, Goodman, and Marcuse, among others, it merits careful reading. "Politicizing the Sexual Problem of Youth," which first appeared in 1932, seems rather dated today despite the efforts of Ollman to make it appear both topical and relevant. (How many present-day Marxists really believe that "If you want to be rid of your sexual troubles, fight for socialism. Only through socialism can you achieve sexual *joie de vivre*"?) Reich's essay "Dialectical Materialism and Psychoanalysis" is rather heavy going, and the remaining essays are more or less typical of much Marxist writing of the 'thirties, e.g., on the need to arouse the sleeping masses and inspire class consciousness in the labor movement. Perhaps I missed something along the way, but while reading these pieces I was more inclined to join the masses asleep than to arouse them.

The impact of Reich's central thesis about the relationship between capitalism and sexual repression is very different. Here Reich took issue with Freud's position in *Civilization and Its Discontents* (1930) that sexual repression was the inevitable price paid for cultural development, by arguing in accordance with Marxist principles that sexual morality like everything else was determined by economic forces and the material condition of society. Reich argued, in essence that sexual repression was the means by which the capitalist class gained control of the *unconscious* of the masses, and through this control insured that the will to rebel would be too inhibited and crippled ever to assert itself in a decisive manner. Indeed, the masses would never even know that they were being manipulated through the agency of sexual morality, believing, instead, that morality was grounded in religion, health, the preservation of the family, the sanctity of womanhood, etc.

Whatever one may think of Reich's assumption that sexual health is possible only under socialism, it is by no means certain that he was wrong about the relationship between sexual repression and submission to authority. There is some empirical evidence (an unpublished study by Lasswell and Rogow) that individuals who suffer from a variety of psychosexual conflicts are more apt to identify *with* established institutions and disassociate themselves *from* those who challenge such institutions, than individuals who suffer much less or not at all from psychosexual conflicts. In other words, it is possible that sexual repression *does* work in support of the authority institutions of society, whatever the ideology in terms of which they justify themselves. Certainly those ridden by anxiety and guilt do not make good revolutionaries, whether for social justice under capitalism or freedom and democracy under communism.

Were Reich alive today he would insist probably that there is no such person as a good revolutionary, and no such thing as a socialist society. Well before the end of his life Reich had ceased to think of himself as either a Marxist or a psychoanalyst in the usual sense, and instead, according to Charles Rycroft, had become a virulent anti-Communist and fervent admirer of Eisenhower. He apparently believed that his views about sexual repression had been perverted into "a free-for-all fucking epidemic" (Rycroft, *New York Review*, December 4, 1969), and he was lonely, bitter, and more than a little paranoid, to judge from an interview with Reich conducted by Kurt R. Eissler in October, 1952. As Reich remained self-exiled and remote in his Maine retreat, his recollections of the past became distorted, and his thought processes bizarre. Those who create in terrible loneliness, working in isolation from the mainstream or any tributary, are no less likely to hallucinate and lose touch with reality than those who live alone in bare rooms. In the United States, the fate of the dissident typically is not prison or a labor camp, as in the Soviet Union, but a kind of solitary confinement that can break him just as effectively as a term in jail. It is tempting but futile to speculate what might have become of Reich and psychoanalysis had he been allowed to remain within the Freudian circle, or what would have been the effect on Marxism had Reich been regarded by the Third International as a leading theoretician. Perhaps the end would have been the same. In any case, the tragedy of Reich's final years speaks for itself.

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**An Introduction to Positive Political Theory.** By William H. Riker and Peter C. Ordeshook. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973. Pp. 387. $11.95.)

Seen in the broadest possible terms, politics is a social method for generating collective decisions from divergent and often discordant individual preferences. In this respect, it resembles economics. Every basic economics text emphasizes that three major allocative decisions —what to produce, how much to produce, and for whom—can be analyzed as the result of households, firms, and governments choosing to exchange goods and services in a variety of organizational settings that combine to form the web of interactive relationships abstractly called economic markets. Drawing much of their inspiration and specific technique from scholars working on the ragged seam dividing politics from economics, Professors Riker and Ordeshook view the study of politics as arid and intellectually uninteresting unless it incorporates a theory of rational choice as a set of fundamental axioms from which more specialized theoretical propositions may be deduced. *An Introduction to Positive Political Theory* springs from the premise that human beings consciously strive to produce collective outcomes at least broadly consistent with their personal preferences. Without this crucial premise, Riker and Ordeshook claim, political science can never be more than a loose amalgamation of weak empirical correlations which present political activity as the mechanical reflex of class position, childhood training, or social location in various hierarchies or groups. Whatever theory emerges from this inductive method they hold, is nothing more than a compendious restatement of the latest empirical findings.

As might be expected from the books and articles they have previously published, Riker and Ordeshook are preoccupied with collecting and integrating bits and snippets of theories dealing with the summation and transformation of individual preference orderings into authoritative collective decisions having the force of law. (There is nothing novel about this theoretical focus; it has been a staple of political theory since Plato.) In my judgment, these chapters form the most solid and substantial portion of the book. The chapter dealing with Arrow's paradox is the clearest exposition of the General Impossibility Theorem I have ever seen in print. Although their conclusions are somewhat guarded, Riker and Ordeshook suggest that attempts to guarantee the transitivity of social choices by organizing vote-selling markets (logrolling) are likely to misfire. Logrolling is methodologically indefensible unless one introduces cardinal utility numbers which can be interpersonally compared and summed. They are skeptical of the von Neumann-Morgenstern technique for transforming an ordered set of preference ranking into an interval scale measuring *intensities* of preference, a point of view which I share. It is important to note that the rejection of cardinal utility numbers renders the use of mixed strategies in game-theoretical formulations questionable as well.

In a brief review, one must forego critique for summary. Riker and Ordeshook present an especially rich variety of models and theories developed by scholars dealing with cooperative and noncooperative games, the calculation of power indices in voting bodies where electors constitutionally cast unequal numbers of votes, electoral competition in two- and multi-party systems, and processes of coalition formation in legislative bodies and international conflict. Their treatment of each topic is deft and subtle, and, where the data permit, Riker and Ordeshook are able to marshal an impressive volume of evidence to test the implications of their theoretical structures.

In treating policy outputs, the authors trenchantly analyze a number of contributions in the rapidly developing field concerned with public goods (or public "bads"), satisfactions or dissatisfactions produced in such a way that no one can be excluded from consuming them. Their handling of the "free rider" problem resulting from the indivisibilities inherent in public goods is standard but lucid. In the absence of coercion or pleasurable selective incentives, all will try to seize the satisfactions derived from consuming the good at the same time they attempt to shift the costs of providing it onto others. One result may be that the good will be supplied at a suboptimal level—how many people would voluntarily tax themselves to finance a fire department or a public park? Paradoxically, with governmental coercion, the polity may produce *more* of a public good than most people prefer. One homely example is a city so oversupplied with public roads that it becomes crowded, noisy, and smelly. Those trapped in the immediate neighborhood are made to suffer uncompensated adverse effects.

A definitive survey is always destined to be staked out as a definitive target, a sure sign of its importance. Political scientists who have been trained to view empirical testability as the prime requisite for intellectually respectable theory are likely to scorn the book as an elegant but misbegotten exercise in the manipulation of factually uninformative symbols. For most of the book, it is true, the correspondence between theoretical and observational languages is disappointingly tenuous. More serious, in my view, is the frequent failure of Riker and Ordeshook to specify evidence that would falsify their theoretical implications. In some cases, this omission invites the conclusion that the theories they present are actually pseudo-theories, vacuous tautologies consistent with whatever facts are discovered. For example, Olson's standard theory predicts that large latent groups will receive few collective benefits from governmental action. If they in fact become the beneficiaries of governmental action, the deviation from Olson's theory can be explained away by endowing legislative entrepreneurs with heroic propensities to assume large risks (pp. 75–76). If, however, the benefits do not materialize, it is safe to conclude that legislative politicians are risk-averse. But, since the only evidence of a politician's appetite for risk are his actions in pressing for legislation, the theory manages to spin out of every counter-argument on its own internal circularity. What the formulation lacks is independent evidence of a legislator's taste for risk, a variable that is entirely exogenous to the theory.

Right off the mark, Riker and Ordeshook aver that their book is intended as an introduction "for students at the junior-senior and graduate levels" (p. xi). I am sorry, but I do not believe that statement for even a moment. My hunch is that the book will be rough sledding for most professors. It bristles with formidable talk of Lagrange multipliers and indifference contours, matters that are generally beyond the grasp of at least 95 per cent of all the incoming graduate students I have known. I hope my guess is wrong, because *An Introduction to Positive Political Theory* is far too rich and imaginative to merit only coterie readership.

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**Fascism in France: The Case of Maurice Barrès.** By Robert Soucy. (Berkeley: University of California Press, 1972. Pp. vii, 350. $15.75.)

Maurice Barrès (1862–1923) is generally regarded as a conservative, a traditionalist, and one of the leading exponents of French nationalism during the two decades preceding World War I. During the war he was a fervent propagandist for the French cause; but after the war his reputation suffered an eclipse from which it has never recovered. Since his literary and political careers were antecedent to the epoch of Hitler and Mussolini, Barrès has not usually been described as a fascist. And yet the title of Soucy's book implies that he should be classified as a fascist of sorts. In the author's view, "A generation before Mussolini and Hitler came to power, Barrès, along with others, anticipated a cluster of fascist values, sentiments, and doctrines" (p. 313). As one follows Soucy's exposition, it becomes clear that the author finds affinities not just between the thought of Barrès and that of such French fascists as Pierre Drieu La Rochelle and Robert Brasillach, but between Barrèsism and Hitlerism as well. The latter connection is particularly emphasized in the chapter on "Vitalism, Massendemocratie, and Racism." But when he then explores at length the similarities between Barrèsism and Pétainism—and Gaullism for that matter—it becomes clear that he has his own conceptions about the pervasiveness of fascist ideology.

Soucy, in the course of this work, seems to imply that Ernst Nolte in *Three Faces of Fascism* (New York: Holt, Rinehart and Winston, 1966) was mistaken in his choice of Maurras rather than Barrès as one of the principle fascist prototypes. Indeed, Nolte gives little attention to Barrès in his monumental study and suggests that if Barrès kept his political distance from Maurras it was because of his inability to accept the narrow-mindedness of Maurras and his fear that Maurras was preaching a doctrine of enslavement.

Soucy opens up a controversy, then, not only about who was the real *maître* of French fascism, but also about the role of Barrès in the genesis of European fascist ideology. The author, to be sure, notes certain dissimilarities between the thought of Barrès and that of the Nazis, of Pétain, and of de Gaulle, but the main thrust of his argument seems clear. Soucy, in a series of articles and now in this work, has undertaken to demonstrate that fascism and conservatism in France, while not completely identical, were more similar than different. It is in this light that his references to his subject's nationalism, conservatism, traditionalism, and national-socialism are to be understood. The fact that Barrès was something of an intellectual vagabond, that he moved from *Boulangisme* to a kind of national-socialism, then to traditionalist conservatism, and finally to Catholic mysticism, poses few problems for Soucy in his analysis of the similarities between Barrèsism and fascism. Indeed it seems to be his major purpose to take up a position in opposition to such writers as René Rémond and Paul Sérant, who see a fundamental difference between French fascism and conservatism.

Some of the French fascists, and particularly the literary fascists, in the nineteen thirties occasionally referred to Barrès as one of their mentors. They also attempted to annex Fourier, Proudhon, and even Péguy. But, in all such eforts, they were eagerly trying to demonstrate that, far from being only the servants of foreign powers, they were in step with an authentically French tradition. After quoting extensively from writings of Drieu and Brasillach (and Maurice Bardèche) that seem to coincide with aspects of the thought of Barrès, Soucy does point to the fact that Barrès "reconciled himself more and more to bourgeois society, becoming one of its staunchest defenders" (p. 300). He is right in acknowledging how far Barrès was from the French fascists on the question of the bourgeoisie, but this in itself would seem to be of capital importance in distinguishing conservatives from fascists.

To say that the thought of Barrès had a curiously diffused impact on Frenchmen of a variety of persuasions would have sufficed. But to argue that he was a major intellectual forerunner of fascism raises further questions about the nature of generic fascism that this book will not put to rest. Soucy seems intent on proving a thesis, but whether he is entirely successful or not will depend on the reader's preconceptions concerning fascism. His book is well researched and he has an admirable, straightforward style despite an annoying repetition of the adjective "tough-minded." Still, Henry Ashby Turner's hypothesis that the term "fascism," used generically, serves more to confuse than to enlighten and that it therefore might be abandoned altogether, irresistibly comes to mind.

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**Main Trends of Research in the Social and Human Sciences.** Edited by UNESCO. (Paris and The Hague: Mouton-UNESCO, 1970. Pp. 819. $30.00.)

By arranging for the compilation, editing, and publishing of this monumental volume, UNESCO has provided scholars with an extraordinarily informative and useful overview and critical survey of a broad field of knowledge, together with impressive evidence of limited but significant progress and growth in much of it.

Large as it is, the present volume is limited in the main to only six of the social sciences, each of which is treated in a substantial chapter: *sociology* (105 pages) by P. F. Lazarsfeld; *political science* (59 pages) by W. J. M. Mackenzie; *psychology* (58 pages) by Jean Piaget; *economics* (168 pages), begun by the late Oscar Lange, continued after Lange's death for a time by W. Brus and two other Polish scholars, and eventually completed by the anonymous staff of the UNESCO Secretariat; *demography* (68 pages) by Jean Bourgeois-Pichat; and *linguistics* (44 pages) by Roman Jakobson. As can be seen, economics and sociology together make up about two-thirds of the total.

A particular strength of the volume is the attention devoted to problems common to the social sciences. This begins with an extremely worthwhile 60-page "Introduction" by Jean Piaget on "The Place of the Sciences of Man in the System of Sciences"; and it continues throughout the second part of the book, which comprises four substantial chapters dealing with interdisciplinary problems. In Chapter 7, Jean Piaget continues his major intellectual contribution to the entire work by discussing "General Problems of Interdisciplinary Research and Common Mechanisms."

In the next chapter, one of the most valuable of the entire book, Raymond Boudon deals with great competence and insight with "Mathematical Models and Methods." Pierre de Bie follows in Chapter 9 with a treatment of problem-focused research; and Stein Rokkan contributes a valuable chapter on "Cross-Cultural, Cross-Societal and Cross-National Research," in which he manages to condense a great deal of thought, research and experience.

The third and final part of the volume consists of a single massive chapter of 120 pages by Eric Trist on "The Organization and Financing of Research." This chapter is perhaps particularly important because the entire volume, in the words of the Director-General of UNESCO, René Maheu, is intended not only for the educated reader, for research workers themselves, and for their professional associations, but also *for those institutions*, both national and international, whose task it is *to organize and finance scientific research* (p. x; italics supplied). In this sense, the UNESCO volume is also a bid to exercise some power and control over social scientists and their work, in favor of such urgent social needs and promising intellectual trends as are perceived by the authors.

The entire volume is intended to deal only with law-seeking or nomothetic sciences of man, which seek verifiable and cumulative knowledge. A second volume, presumably of similar format, is to follow and to deal with the normative, historic, philosophic, aesthetic, and generally humanistic approaches to the study of man, in which the values and personality of the investigator tend to play a much larger part. It is to be hoped that those parts and practitioners of political science who are working in this predominantly humanistic tradition of our field—it is an ancient and honorable one—will be represented by a chapter in that future volume, too.

Among the many issues raised by a volume of this size and scope, three stand out: (1) the general intellectual merit of this work; (2) the particular quality of its treatment of political sciences; and (3) the possible implications of its line of reasoning on social science policy and financing.

As to the first point, the intellectual quality of the book is remarkably high. This is by no means the usual fat compendium held together by little but the efforts of the bindery. On the contrary, it is a serious, first-rate work full of highly relevant ideas. The intellectual influence of Jean Piaget appears in many places throughout the book, in addition to the three chapters he has written for it. Together with the contributions by Raymond Boudon and Paul F. Lazarsfeld, these chapters would make the book even at its high price a bargain.

The long chapter on economics seems to offer the noneconomist a very useful and open-minded overview of economic doctrines, problems, and methods of research and analysis; and many of the points it raises—such as the still persisting lack of an adequate theory of economic development—should prove challenging and stimulating also to professional economists. A particular contribution of the chapter on economics is its undogmatic discussion of different schools of economic thought, including both non-Marxist and Marxist views and treating them in a calm, intelligent, and problem-oriented manner. It exemplifies the kind of significant intellectual contribution an organization such as UNESCO can make, and it reminds us that some of the greatest problems facing mankind are not those that divide East from West and North from South, but those that all of us have in common.

In the panorama of social sciences, as presented in this volume, political science has been allocated a very modest place. Not only the limited number of pages but also the lists of consultants and collaborators bear this out. Among these in sociology, we find many active researchers and theorists, such as Robert Angell, François Bourricaud, Ralph Dahrendorf, K. Davis, S. N. Eisenstadt, Johan Galtung, T. H. Marshall, Talcott Parsons, Neil Smelser, Otto Stammer, and Wolfgang Zapf; in psychology, there are Marie Jahoda, Otto Klineberg, Charles Osgood, Jean Stoetzel; but in political science there are only two names with similar records of major substantive contributions to their field, Raymond Aron and Stein Rokkan, both of them also claimed by the profession of sociology. This is compensated only in part by the inclusion of a number of meritorious functionaries of political science organizations, such as the Secretary-General of the International Political Science Association, André Philippart; of his predecessor, Serge Hurtig; of such leading functionaries of French political science organizations or institutions as J. Meyriat and the late J. Touchard; and of some respected scholars from smaller or non-Western countries, such as J. Steiner from Switzerland and R. N. Trivedi from India. My list is incomplete, and I apologize in advance for the possible omission of the name of some fine scholar, but the contrast to the broader representation given in this volume to other fields remains manifest. It is much less a reflection of our field than of the limitation of UNESCO's consulting process in this instance.

An additional peculiarity is the almost complete absence among the UNESCO consultants of political scientists from the United States. Since more than one-half of the world's political scientists professionally engaged in research or teaching are in the United States, their absence from, or extreme scarcity among, the collaborators listed seems somewhat surprising.

Under these conditions it is doubly impressive that a British scholar, Professor W. J. M. Mackenzie of the University of Manchester, has written as excellent a chapter on political science as we have in this book. His treatment is broad, tolerant, shrewdly critical, and eminently fair. Throughout, there is a stress on the unity of our discipline, not as a matter of pious hopes but as one of instance after instance of demonstrated intellectual cross-connections and interdependence among the different analytic perspectives and aproaches. An enterprising publisher could do a great deal worse than reprint this chapter as a brief paperback pamphlet.

Within this overall positive appraisal, some critical points deserve to be raised. The author stresses "three major changes which are irreversible": (1) political scientists have learned the language of variables; (2) they are learning "that models—if they are used—should be used scrupulously," but that "the magic resides in rigor, not in symbolism"; and (3) there is a new appreciation of the importance of quantitative data which "can be stored and analyzed electronically," even though "serious problems" remain about their reliability and completeness. Nonetheless, Professor Mackenzie also seems to accept the late 19th-century distinction between "nomothetic" or law-seeking sciences, such as the "natural" sciences, on the one hand, and "idiographic," or quality and uniqueness-oriented disciplines, such as the "cultural sciences" or humanities, on the other. The distinction was proposed by the German scientists Heinrich Rickert and Wilhelm Windelband in the late 19th century, and effectively criticized by the British historian R. G. Collingwood in his book *The Idea of History* in 1945. One wishes that Professor Mackenzie had found space to tell his readers about this criticism, and about the opposing viewpoint of the "unity of science" movement of the 1930s and 1940s.

Finally, Eric Trist's recommendation in the last chapter of the volume for science policy and the channeling of financial support should be read with great but critical care. After presenting a variety of data from various countries, Trist concludes that "academic individualism" is outmoded. The future, one gathers, should belong to larger teams and organizations, avoiding "duplication" of research and directing their efforts according to plan and in response to the wishes of their financial supporters and their clientele. As the problems to be studied become larger and more complex, he argues, they will transcend the capacities of a single human mind, and social scientists will have to form large teams, and their work will have to be supported for decades. All this, in turn, will make social scientists more dependent on support by governments or clients and make "scientific individualism" still more obsolete.

These arguments, in my view, mix insights and oversights. What is the frequency and distribution of errors made by planners of research—by individuals, by committees, by bureaucracies, and by the opinion of large groups? If these error rates are not negligible, what will be the probable results of pyramiding one decision upon another, as is often inevitable in research? Might it be sensible to budget some proportion of resources for "nonobvious" research projects—perhaps those endorsed by at least one-third, but by less than one-half, the members of a well-qualified reviewing board? Under the system of "academic individualism," nonobvious or currently unfashionable lines of inquiry still have some chance of being pursued. How would they fare under a system of centrally planned priorities, or of increased client's "co-determination"?

There are as yet few conclusive answers to these questions. We cannot accept Mr. Trist's prescription, I think, as it stands, but he does give us, here and now, much food for thought.

So, clearly, does the entire book. As a whole, it is an important contribution. No respectable library should be without it; and any social scientist or general student of human nature, if temporarily in funds, might consider it as a valuable investment.

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**Regicide and Revolution: Speeches at the Trial of Louis XVI.** Edited with an Introduction by Michael Walzer. Cambridge Studies in the History and Theory of Politics. (New York: Cambridge University Press, 1974. Pp. 219. $12.50.)

Why should we care today about the judicial murder of a king almost two hundred years ago? Michael Walzer gives us several reasons in the long and thoughtful essay which opens this book.

Professor Walzer argues that the renunciation of ancient authority requires a clear symbolic action of its own, and that the founding of a new regime is unlikely to be successful unless the old has been put formally and publicly to rest, not just swept quietly into a corner. "Public regicide," he says, "is an absolutely decisive way of breaking with the myths of the old regime, and it is for this very reason, the founding act of the new" (p. 5). Such decisiveness, he asserts persuasively, is essential if a people is to recognize and accept that they have renounced their servitude and taken on new responsibilities.

According to Walzer, France and England, by breaking boldly with the past through the formal killing of the king, prepared themselves better for their political future than did peoples who dealt with kings in a less clearcut fashion. He raises some interesting questions about political psychology, the uses and powers of myths in politics, and the ways in which they can be finally retired. He also, with his customary wit and elegance, makes us take seriously some rather archaic problems in political procedure: How indeed does a people go about killing a king legally and formally, with appropriate ritual and some sort of legitimacy to grace the act? Finding the language and the laws, the proper accusations and the proper punishment, was a most delicate job for the French as it had been for Cromwell's men the century before.

Walzer, well versed in the behavior of the Puritans, draws comparisons between the trial of Louis and the trial of Charles Stuart, and uses both events as a basis for his generalizations. He gives a clear idea of the extraordinary audacity and political imagination it took for Cromwell and the People of England to say "We will cut off his head with the crown on it" and then carry off the deed. The distinction between the public and private capacities of the king, familiar for so long, was never more important than at that moment. To kill a king in private, to smother a man in a tower or stab him in the road—all this was commonplace. But to kill the kingship and not just the king was quite a different matter.

When one has finished Walzer's essay one has the sense of having learned much about the ways in which a people come to terms with their most powerful public symbols. The speeches themselves, ably translated by Marian Rothstein, are less rewarding. There is much rhetoric about the blood of tyrants, much screwing up of courage so that the deed may be done, a certain air of whistling in the dark, but very little theory. This is what we might expect of men in such a situation, I suppose. But given the long and carefully protracted tradition of discourse in France for several centuries about the origins and limits (if any) on the kingship, one might have hoped the French would do a better job of ornamenting with philosophy, or at least with finer language, what they wanted to do anyway. They carefully skirted, for the most part, the major issues. What right had they, this particular group of men, to be pronouncing sentence anyway? For whom did they speak, and how did they relate to that mystical entity, the French People? For what, on what charges, was the king being tried? For treason? Against whom, and in what form? For pillaging and cheating and making war to defend himself? Or only, after all, for being king?

This of course was what it came to in the end, as Walzer recognizes. The deputies themselves could hardly be expected to say it openly. To justify killing Louis they had to strip him of his divinity. Destroying the kingship was such a radical act, it seems, that to be able to perform it the deputies had to move violently from one extreme to the other. The king had been thought almost a god; now he must be made to seem a monster, less than human. He must be decked out in the most horrid colours, charged with the most awful crimes, his hands shown to be stained with blood, in order to be a fit object for execution.

Saint-Just and Robespierre, less squeamish and more direct, if no more logical, than their colleagues, were readier to talk about what was really going on. Robespierre put it this way: "Regretfully I speak this fatal truth—Louis must die because the nation must live" (p. 138). While Louis lived, the fragile new republic could not hope for tranquillity or for safety; these things required first the punishment of the tyrant. Saint-Just put it even more succinctly: "The Revolution begins when the tyrant ends" (p. 176).

It is hard to avoid drawing parallels between France in 1792 and America today. The useful comparative lessons for the most part have to do with political psychology and the ways people learn to understand and change their sacred symbols. This is one reason why Walzer's essay is so thought-provoking. More specific parallels are few, but there is a delightful irony about the homespun speech by Tom Paine which closes this selection. Paine argues that Louis though guilty not be killed, but exiled to America. "Let then those United States be the safeguard and asylum of Louis Capet. There, hereafter, far removed from the miseries and crimes of royalty, he may learn, from the constant aspect of public prosperity, that the true system of government consists not in kings, but in fair, equal and honourable representation" (p. 212). *Sic semper tyrannis.*

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**The Power Motive.** By David G. Winter. (New York: Free Press, 1973. Pp. 373. $12.00.)

Psychologists and political scientists have common concerns but dissimilar approaches. This is well illustrated by Professor Winter's book, which seeks to do for power what David McClelland has done for achievement. Winter's stated aim is to identify, measure and understand the power motive. He approaches the task from an empirical rather than a conceptual perspective, seeking, as he puts it, to avoid semantic questions and to "map the range of actions correlated with the power motive." By the standards of his discipline, he identifies and measures well. There will be those, however, who question the extent to which he understands.

The early chapters set forth Winter's conception of power (the capacity of one person to produce consciously or unconsciously intended effects on the behavior of another person) and his strategy for measuring it (recording and classifying the images and themes people set down in TAT protocols after the power motive has been experimentally aroused). These chapters are models of how a psychologist goes about explicating and operationalizing a concept. They also reveal Winter's penchant for exhaustively detailing all of the alternatives considered prior to each major research decision. His strategy, which admittedly has intuitive and logical appeal, is first to develop a cautiously constructed and elaborately validated measure of the power motive (chapter three). The next step is to correlate this measure with a wide variety of behaviors and characteristics to uncover those most closely related to the power motive (done largely with data collected from Harvard and Wesleyan undergraduates, chapters four and five). Finally, the strategy calls for using these empirically established patterns of thought and action as guides to refining the definition of power (chapter seven). Winter performs the first two operations admirably, but the third receives short shrift. He never really pulls all the empirical threads together into an integrated conception of the power motive, which is not surprising given the comparatively little attention he devotes to the effort.

Many of Winter's research findings are of interest to political scientists. Among students, he finds significant correlations between the strength of the power motive and participation in competitive athletics, and the tendency to seek and hold campus political offices. Among adults, however, he found little difference between aspirants to political office and nonpolitical control groups as regards the strength of the power motive. Winter finds this surprising, and concludes that the traditional assumptions concerning the attraction of politics for the power-hungry may require modification. In a related study, Winter correlated n power with career choice among samples of Wesleyan and Harvard undergraduates. He found, surprisingly, that students high in n power are not especially drawn to government, politics or law, but prefer careers in teaching, psychology or the clergy. He suggests that this might be explained by the greater opportunities for individual autonomy such careers offer. Winter also attempts to discern, from his own and others' data, patterns in the strategies followed by high n-power people as they search for power in various settings. The data suggest that the interpersonal styles of such people include a proficiency in becoming well-known, a capacity for forming useful alliances, and a competitive disposition toward other power-oriented people.

One of Winter's most creative and interesting undertakings is his analysis of the legend of Don Juan as a device for generating additional hypotheses about correlates of the power motive. The legend is seen as an archetype of the power motive, in that it embodies such dramatic power themes as seduction, murder, challenge to honor and glory. After culling the legend for power imagery, Winter reports elements of his student data which pertain to the themes, e.g., those high in n power report greater frequency of sexual contact than others, and have a preference for unassertive, dependent wives. They are also resistant to illusions created by others, and report a greater desire for career autonomy, and generally for avoiding the dominance of others. At a sociopolitical level, Winter finds a moderate correlation between a country's production of Don Juan style stories and engagement in wars.

Winter's concluding efforts to examine the power motive in social and political contexts are disjointed but suggestive. He argues for example, that motive levels can affect the direction and characteristic orientation of socially or politically active groups, reporting data which suggests that high n power groups like black militants seek to make the system work for them, and hence support the system, while low n power, high n achievement white radicals tend to seek fundamental changes in values, and hence the potential overthrow of the system. Winter also re-examines his and Donley's coding of power and achievement imagery in presidential inaugural speeches. He notes a high correlation between need for power and need for achievement, and suggests it is an outgrowth of the political system. This is because candidates who win the Democratic nomination have usually possessed high levels of both motives, while success as a Republican has generally implied low intensity in both motives. The system, he argues, tends to freeze out those high in one motive but low in the other.

This book will be of interest to those political scientists concerned with the psychology of power and with techniques for assessing its strength as a human motive. Those seeking an integrated conceptual treatment of power however, are advised to look elsewhere. For an important shortcoming is that in seeking the broadest range of empirical correlates of power, Winter pays a price in depth and cohesion. The work lacks integration. The disparate power correlates are never systematically pieced into a meaningful theoretical mosaic.

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**5 Public Philosophies of Walter Lippmann.** By Benjamin F. Wright. (Austin: University of Texas Press, 1973. Pp. 171. $6.75.)

In this little volume Professor Benjamin F. Wright has neatly, if at times prosaically, summarized and criticized nine of Walter Lippmann's books that are of special interest to the political philosopher. His central argument is that—in these nine (*A Preface to Politics*, 1913; *Drift and Mastery*, 1914; *Public Opinion*, 1922; *The Phantom Public*, 1925; *A Preface to Morals*, 1929; *The Method of Freedom*, 1934; *The New Imperative*, 1935; *The Good Society*, 1937; and *Essays in the Public Philosophy*, 1955)—Lippmann offers five distinct points of view concerning such issues as the capabilities of political man, the functions of government and laws (human and natural), the concept of property, the role of science, and the uses of history. Wright outlines Lippmann's assumptions, methods and conclusions as the latter moves from an optimistic progressive in the mold of Theodore Roosevelt and Herbert Croly, through a searching examination of the nature and role of public opinion, from a brief demotion of the role of government to a broker or mediator in *A Preface to Morals* to an enthusiastic endorsement of the first two years of the New Deal, then to a critique of FDR's policies in *The Good Society*, and finally to an almost nostalgic defense of the tradition of civility and natural law in *The Public Philosophy*.

Though Professor Wright concludes with the caveat that consistency in thinking about politics is not necessarily a virtue, he nonetheless insists that Lippmann's works on political theory contain so many "interstitial variations, modifications, and reversals that it is difficult, perhaps impossible, to find adherence to any discernible basis for formulating a philosophy of politics that deserves the respect and the standing easily accorded to numerous portions of the books" (p. 135). There would be obvious benefits in seeking to understand why Lippmann modified his assumptions, shifted his focus, or changed his priorities during the forty-two years spanned by the nine volumes, but Wright has not analyzed possible explanations. Rather he has approached his subject as though Lippmann were primarily a theorist rather than a political journalist. Walter Lippmann has, without doubt, aspired to contribute to our thinking about the problems of liberal democracy, and these nine books should therefore be measured against the major contributors to American political thought including Madison, Jefferson, and Calhoun as well as the most impressive among his contemporaries. But to find him deficient because he has been less consistent, less accurate in his interpretations of history and literature, and less eloquent or substantive in his rhetorical flourishes than the Founding Fathers, is to give him less than his due as one of the most significant American political thinkers of the twentieth century.

Wright concedes that Lippmann is "the most versatile, the most wide ranging, and in some respects the most impressive" American political thinker of this century (p. 9). He finds him "the most interesting political theorist of his time," though not necessarily the most influential (p. 157). Influence in the realm of ideas usually requires a clear and consistent theme articulated over many years; it also demands that the commentator focus primarily on the dominant public concern of his era. Walter Lippmann does not meet either of these requirements. He has adjusted his focus and even his conclusions as time and events impressed him with the need to modify earlier views. No doubt he would respond to Professor Wright's complaints as he reacted to the experience of reading, during a late summer evening in 1915, an unnamed author of "one of those books on politics which are regarded as essential to any sort of intellectual respectability." In his imaginary dialogue with the pretentious and prosaic author, he observed: "You are drawing sketches in the sand which the sea will wash away . . . you apologize for your timidity by frowning learnedly on anyone who honestly regards thoughts as an adventure, who strikes ahead and takes his chances. You are like a man . . . trying too hard to make a good mashie shot in golf. It can't be done by trying so hard to do it. Whatever truth you contribute to the world will be one lucky shot in a thousand misses. You cannot be right by holding your breath and taking precautions." ("Books and Things," *The New Republic,* 4 [August 7, 1915], p. 24).

By taking "a number of shots" at most of the central problems of political philosophy, Walter Lippmann has probably come closer to the truth than those who have carefully calculated their few dramatic attempts. Examine, for instance, his ambivalent and shifting views on the capabilities of political man: an unambiguous view of the role of human nature in political affairs would be a naive one. Some men, individually and collectively, have demonstrated their potential for sensitivity, compassion, and creativity while others have shown their propensities for irrationality, inhumanity, and destructiveness. The same ambiguity is no doubt justified in defining the functions of government and laws. Different eras demand different reactions from executives and legislators. In times of economic crisis or violent social conflict, the active role advocated in *The Method of Freedom* and *The New Imperative* will be necessary; following the resolution of such crises, a lower governmental profile—that of an occasional broker or mediator among private interests as proposed in *A Preface to Morals* and *The Good Society*—will probably be in order. Although he has not explicitly justified his shifting views of political man and the functions of government in this fashion, the style and method of Walter Lippmann's commentaries on politics suggest that this rationale of "constructive ambivalence" might well appeal to him.

James Lare

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**John Locke: Problems and Perspectives.** Edited by John W. Yolton. (Cambridge: Cambridge University Press, 1969. Pp. 278. $14.50.)

The expanding community of Locke scholars should be gratified by the appearance of a volume such as this. It provides welcome evidence of a vigorous exchange of views among five British, five North American, and two continental scholars representing a broad range of disciplines. Collectively, the contributors to this volume approach Locke's work in its totality, rather than from within the Balkanized and zealously guarded frontiers of contemporary academic disciplines. They are generally well suited by training and experience to explore the relationships between the political, educational, religious, economic, epistemological, and other aspects of Locke's writings. Included among the essayists are four political scientists: Richard Ashcraft, Peter Laslett, Gordon Schochet, and M. Seliger; three historians: James Axtell, Esmond S. De Beer, and John Dunn; two professors of English: Hans Aarsleff, and Rosalie Colie; and three professors of philosophy: Raymond Polin, W. Von Leyden, and John Yolton. The genesis of the volume was a conference on "The Thought of John Locke," but it should be emphasized that it does not consist of unedited conference presentations, as do far too many publications of this type. The conference papers included in the volume were reworked for publication, while additional essays were commissioned by the editor, Professor Yolton, whose reputation for high standards of scholarship is attested by his contribution to this volume (pp. 183–193).

Given limitations of space, it seems appropriate to concentrate here on those essays that focus most directly on the concerns of political scientists. These essays especially remind one that the analysis of Locke has given rise to extraordinarily sharp controversy since his major works were published—mostly anonymously—late in the 17th century. Fresh and significant evidence that Locke anticipated such controversy is provided by Dr. De Beer, who provides an illuminating account of the extreme caution he took to conceal his authorship of the *Two Treatises of Government*. Locke "not only evaded his friends' inquiries; the extant correspondence . . . suggests that he was at pains to conceal his authorship from the publisher, Awnsham Churchill . . . [who] was not to see a scrap of writing in Locke's hand . . ." (p. 35).

Locke's efforts to avoid controversy proved unavailing. His "strange doctrines" provoked the dreaded "storm" of criticism to which he referred in his reply to the Bishop of Worchester, who charged that his " 'new way of ideas' undermined the principles of Christianity" (p. 198). Professor Ashcraft suggests that "The general consensus of opinion among those who have read the *Essay Concerning Human Understanding* in the last two centuries would probably agree with the bishop as to the subversive character of Locke's exploration of the foundations of religious belief" (p. 194). No less a friend of Locke's than Sir Isaac Newton attacked him in a letter in which, as Newton subsequently explained, "I took you for a Hobbist" (p. 199, n. 2). The charge of Hobbism was especially threatening, since, as Dr. De Beer reminds us, by "1680 *Leviathan* was completely discredited; . . . the book was an atheistic vindication of Cromwell" (p. 40). Locke always stoutly denied that he was even "well read in Hobbes,"—much less influenced by him, but attacks based on what might be termed the "hard" perspective or interpretation of his work continued during his lifetime and throughout the 18th century (p. 45).

Granting that this "hard" perspective may not provide an adequate understanding of Locke's intention, it is nevertheless striking how little serious attention it receives from most contemporary interpreters of Locke, most notably the essayists who have contributed to this collection. They thereby severely limit the scope of their interpretation. Still, to give them their due, one must observe that it is Locke himself who provided the basis for the currently dominant interpretation of his work. Locke's writings are replete with quotations from scripture and his professions of Christian piety can hardly fail to capture the attention of even the most superficial readers. One is therefore hardly surprised to observe that Professor Polin in his essay on Locke's "conception of freedom" observes that "The metaphysical and even theological dogma of eternal salvation constitutes indeed the foundation and the right justification for the existence of freedom" (p. 4). Professor Ashcraft advances this position with his suggestion that "All the roads of Lockean philosophy lead to the hallowed ground of Christianity" (p. 214). Ashcraft finds that Locke occasionally encounters obstacles on this road, but they do not prove to be insurmountable. For when "the epistemological views of Locke, the Christian, cannot be satisfactorily reconciled with those of Locke, the philosopher, it is the faith of the former which ensures the salvation of the latter" (p. 223). Indeed, "at critical points in the exploration of human ignorance" in his *Essay Concerning Human Understanding*, "Locke speaks less as a philosopher than as a prophet . . ." (p. 204).

Writing in a somewhat different vein, Professor Schochet attempts to establish the interesting thesis that "an important and persistent acceptance of what is at base Aristotelianism can be extracted from Locke's conceptions of society and government . . ." (p. 89). His failure to explain what he understands here as "Aristotelianism" makes it difficult to evaluate his argument. Aristotle would hardly have settled for the extremely vague proposition that man is "*somewhat* sociable by nature," (p. 92, emphasis added), or have accepted the view that "By looking at man in his original, pre-political condition" one could properly "determine what kind of government would be most consistent with human nature," as Locke is said to have done (*Essay Concerning Human Understanding*).

Schochet also places considerable reliance on the notion that "the doctrine of trust was one of the most important components of the theory of the *Two Treatises*" (p. 98). But he fails to develop the grounds for this view. His attribution to Locke of the position that men, even outside of civil society, were "other-regarding rather than hedonistic" (p. 96) stems from his questionable identification of the state of nature and the character of rule in primitive or undeveloped communities.

Many of the foregoing themes receive their most careful and complete development in Professor Aarsleff's two substantial contributions to this volume. The tone of Aarsleff's argument is suggested by his contention that "for Locke the Bible was the revealed word and as such the ultimate authority in all human matters. To believe otherwise is to ignore the plainest evidence in all of Locke's writings" (p. 130, n. 1). Aarsleff rightly assures us however that "Locke had . . . strong reasons for not wishing to found his social and political philosophy on special revelation" (p. 105), and he finds in his work "complete harmony" between "two kinds of revelation, special and manifest . . ." (p. 105) or "natural *revelation*, whereby the eternal Father of light and foundation of all knowledge communicates to mankind that portion of truth which he has laid within the reach of their natural faculties" (p. 107). Aarsleff agrees explicitly with a portion of Polin's analysis (see p. 110, n. 4) and with Schochet's finding that Locke is not a hedonist (pp. 111–116). He argues that through the use of their God-given reason some parts of mankind have always been able to overcome the promptings of the passions to gratify their immediate desires and thereby to achieve true happiness. This is nothing less than "the exquisite and endless happiness" that is the anticipated reward of those who have earned Eternal Salvation, as Locke tells us in the *Essay*.

But what is the status of Eternal Salvation in Locke's teaching? Aarsleff's inspiring interpretation depends decisively on whether Locke has demonstrated the immortality of the soul. Unfortunately, according to Locke, its demonstration is beyond the capacity of human reason. The doctrine of the immortality of the soul is an "article of revelation" about which human reason can attain "to no clearness, no certainty," as Locke concludes in his famous discussion of this point with the Bishop of Worchester (*The Works of John Locke in Nine Volumes*, 1824, III, 489). The Bishop, a "hard" interpreter of Locke, continued to probe "the subversive character of Locke's exploration of the foundations of religious belief," as Ashcraft has put it. Contemporary interpreters tend to be much more easily satisfied than the Bishop and far more generous with Locke. Thus, Aarsleff appears to help Locke overcome the critical question of the immortality of the soul through nothing less than act of faith: "Absolute proof of the non-existence of a future state is not to be had, consequently the knowledge of the mere probability of a future state with rewards and punishments is that which must direct our search for happiness" (p. 116). Whatever judgment one may make of an argument such as this, one must not forget that it was Locke who left the way open for devoted men to understand his work within strikingly different perspectives. They are worlds apart, so to speak, but are useful at least in directing us to reflect on the difference between a political philosopher and a political divine.

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**Public Policy Toward Environment 1973: A Review and Appraisal.** Annals of the New York Academy of Sciences. (New York: New York Academy of Sciences, 1973. Pp. 202. $25.00.)

**Man and His Environment.** By Harry G. Johnson. (Washington, D.C.: British-North American Committee, 1973. Pp. 33. $1.00.)

**Our Ecological Crisis.** By Grahame J. C. Smith, Henry J. Steck, and Gerald Surette. (New York: Macmillan Publishing Co., Inc., 1974. Pp. 198. $5.95, cloth; $2.95, paper.)

As the initial fervor of the environmental movement has waned, the attention of those concerned about environmental policy has increasingly turned to two fundamental questions: How do the many and varied aspects of environmental quality relate to each other and is a livable environment compatible with continued population and economic growth? These questions underlie much of the discussion in the above three publications.

*Our Ecological Crisis* is composed of three essays—one on ecology by Smith, a second on economics by Surette, and a third on politics by Steck. The essays are framed by a cohesive and well written introduction and an epilogue.

The authors of *Our Ecological Crisis* argue that "the environmental crisis is the result of very deep forces within our society and culture" and cannot be easily understood, simply defined, or readily solved (pp. vii–viii). They perceive the central question to be: "Can modern societies—characterized by population increase, by advanced technologies, and by a growth-oriented, man-centered culture—adapt to the constraints of the environment in such a way as to ensure not simply survival, but a decent life for future generations?" (pp. 183–4). They are not optimistic about the answer. "From an ecological perspective, growth, expansion, and domination remain the central sociocultural objectives of most advanced societies and comprise the aspirations of the developing societies" (p. 186), and these objectives and aspirations will lead to disaster for mankind.

Such profound and radical conclusions require profound and radical analysis for support, and two of the three essays provide such analysis. Smith's essay on ecology is the best short summary I have read about why the environmental problem does represent a crisis and how and why doom could result from environmental neglect. He correctly focuses on global long-range problems, identifying the major threats to man's existence as changes in atmospheric composition, radiation, pesticides, toxic metals, and ocean dumping. Each of these problems is sensitively and accurately discussed within an overall ecological framework.

Surette's chapter on economics is a sharp departure from the usual economic analysis of environmental problems. As Surette observes, the standard economic approach is "a logical deduction from given assumptions that are seemingly grounded in the empirical workings of the culture. No serious question is raised about the assumptions, nor are the cultural bases for such assumptions ever examined" (p. 88). Surette does question the assumptions, and his chapter is largely an examination of the cultural basis of the economics of industrial America and how this cultural base affects the market economy and the environment. It is not as cohesive or well argued as the ecology chapter, and Surette is not the first economist to raise these questions (Ken Boulding has been posing the same challenges to economists for many years). But the chapter is insightful, challenging, and follows nicely from the ecological analysis.

Coming after the first two essays, Henry Steck's chapter on politics is a disappointment. Steck limits his analysis almost entirely to air and water pollution and largely to the period before 1970. However, after a useful history of the environmental movement and a discussion of the difficulties of Federal enforcement and standard-setting, he does discuss the organizational and legislative changes which occurred between 1970 and 1972.

Steck's conclusion is that there is an emerging "environmental-industrial complex" whose "preeminence" in environmental policy making is assured (p. 178). "Although it is true that the crisis nature of the environmental situation will continue to produce government action and although the American public will doubtlessly become increasingly alarmed, the possibilities for a meaningful ecological politics hardly seem to exist" (p. 179). The political process is dominated by the forces of big industry and these forces oppose effective environmental action.

This conclusion may be correct, but Steck provides little evidence to support it, and his analysis of how industry exercises such great influence is superficial and misleading. The dominance of the corporations apparently does not manifest itself in legislation. Steck characterizes the 1970 Clean Air Act as "wide-ranging, even radical, in its dimensions and its possibilities" (p. 155) and says that it "represents a major step toward a reordered approach to the environmental crisis" (p. 156). He believes that the 1972 Amendments to the Water Pollution Control Act are "a serious and long-term attempt at cleaning up the nation's waterways" (p. 156) and reflect "a welcome congressional intention to overcome the uncounted failures of the past" (p. 157). The insidious influence of industry also does not show up in the Environmental Protection Agency (EPA), or the Council on Environmental Quality. These organizations provide "the means for a future Administration that may seriously attempt to redress the imbalance between man and his natural environment" (p. 136), and the EPA has "acquired a reputation for independence that most shrewd administrators would covet" (p. 135). Steck does not deal at all with the courts and touches only incidentally on the intricacies of Federal-State-local relationships.

The primary villain at the Federal level, in Steck's mind, seems to be a now-defunct advisory committee called the National Industrial Pollution Control Council (NIPCC). Steck devotes five pages to a discussion of NIPCC. He believes that "it may well have an impact on environmental policy equal to that of any other governmental agency" (p. 167) and that "because of a peak role in the authoritative decision-making process, such high-level advisory bodies as the NIPCC must be regarded as having an influential if not decisive voice" (p. 171). Anyone familiar with environmental policy making in Washington can only regard this as nonsense. NIPCC in fact exercised almost no power. As one measure of its influence, it was not permitted even to see copies of Administration legislation until the bills were made public. In June, 1973, Congress declined to appropriate any funds to support NIPCC. The White House did not wage any battle to save it, and thus the "decisive voice" of NIPCC was effectively and ignominiously silenced.

Harry Johnson's short pamphlet, *Man and His Environment,* is a literate non-technical presentation of the orthodox economist's view of environmental problems. He believes that "alarmist fears of population pressing on natural resources are unjustified" (p. 3) and that "economic growth, and not the impediment or prohibition of it, is the way to betterment of the quality of life and the avoidance of ecological disaster" (p. 27). Perhaps the most fundamental difference between Johnson and the authors of *Our Ecological Crisis* is in the realm of history and science rather than economics. Whereas Smith et al., are inclined to believe that "in the last few years we have passed a pollution threshold in a strictly ecological sense as well as in a sense of awareness" (p. 5), Johnson says that, "[t]here is no reason to think, in the light of millennia of human history and the consistent falsification of the prophesies of successive schools of doom predictors, that at the end of the third quarter of the twentieth century everything has suddenly changed" (p. 13).

*Public Policy Toward Environment 1973* is a collection of twenty very short articles covering almost all aspects of the environment from earthquakes to genetics. The authors are mostly scientists and include such luminaries as Glenn Seaborg, Detlev Bronk, and Kenneth Clark. Although the assignment given to each of the authors included describing the goals of public policy in the area under discussion, giving criteria for judging the adequacy of the policy, evaluating achievement under the policy, and suggesting proposals for an optimum policy, few of the articles actually carry out this assignment. The best articles, such as those on climate control and food plants, deal with problems for which little or no policy exists.

It is very difficult to know who the audience for *Public Policy Toward Environment 1973* is supposed to be. The essays are much too brief and general to be of real relevance to policy makers, and yet their brevity and the lack of coherence of the volume as a whole makes it not very useful to the student or lay reader trying to understand environmental problems. It contains a number of miscellaneous interesting facts and observations, but a better format should be found for injecting the thinking of the scientific community into the policy process.

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**The Politics of Continuity: Maryland Political Parties from 1858 to 1870.** By Jean H. Baker. "The Goucher College Series." (Baltimore, Md.: The Johns Hopkins University Press, 1973. Pp. 239. $11.00.)

Professor Jean H. Baker of Goucher College argues that the disruption of American politics in the 1860s did not produce in Maryland "a durable realignment in both the anatomy and ideology of the Democratic and Republican parties" (p. xiii), but rather the postwar Democratic dominance "merely confirmed changes effected in the late 1850s" (p. xiv). To support this thesis she exploits the major extant manuscript collections, newspaper commentaries, and legislative records, and she makes effective use of the Rice index of cohesion to study legislative roll calls, and limited use of correlation coefficients to determine the relationships of variables and election return data. The result is a refutation of Charles L. Wagandt's contention (1964) that the state's civil war politics represented a "Mighty Revolution" revolving around the issue of Negro emancipation and eventual suffrage.

In the political turmoil of the 1850s which typified virtually all American state and local politics, a group of young Maryland agrarian gentry (personified by Oden Bowie of Prince George's County) gained leadership positions in the Democratic party, successfully developed a series of county committees and legislative caucuses which formed the base of their internal discipline, and then articulated the issues of Negrophobia, nationalism, and local autonomy to secure legislative control by 1858. Their opponents were more likely to be older men who had less wealth and social status, and came from commercial or business backgrounds. Drifting from Whiggery, to Know-Nothingism, to Unionism, to Republicanism, such men were decidedly antagonistic to the ideology of party discipline that characterized the Democrats. This group contained some of the most dramatic personages of Maryland political history. Congressman Henry Winter Davis was a political ideologue whose radicalism is remembered in the famous Wade-Davis reconstruction bill, his protege Senator John A. Creswell became a patronage-hungry partisan hack, Governor Thomas Swann was a political chameleon who successfully tacked his ship of political fortune with each shifting breeze, and Montgomery Blair, Lincoln's Postmaster General, sought to form a conservative coalition from anti-radical Unionists and dissident Democrats. With all of this talent and with the ability to label their opponents as the followers of treason and disunion, Baker finds it amazing that Democratic defeats were so few and their triumphs so great by 1866. The best indicators of the Democratic party's revival were the Constitution of 1867 and the election of Bowie to the governorship with 75 per cent of the vote. Unionist factionalism, Lincoln's heavy-handed deprivation of civil liberties in Maryland, effective local Democratic organization, Negrophobia, and party loyalty, all contributed to a series of victories that culminated with Swann and Blair coming hat-in-hand into the Democratic party.

Despite the importance of her contribution to our understanding of the Free State's political development in the period, Baker makes little substantive addition to the growing sophisticated body of local history of politics during the Civil War era. Admitting that a socioeconomic analysis will not explain voting patterns (see notes on pp. 44, 106, 110), she makes no attempt to test ethnocultural variables other than the number of blacks (for which there is a positive correlation with the Democratic vote). Particular attention should have been paid to the Baltimore city wards that sent Davis to Congress. If this had been done, we might well have had another important study of early American politics like those of Lee Benson on New York (1961) and Ronald Formisano on Michigan (1971). We need to know how such variables as religious preference and national origin influenced voting. Are they as significant in Maryland as they seem to have been in New York and Michigan? Thus, while Professor Baker's work represents a significant contribution to the study of Maryland politics, it does not constitute the definitive analysis of the period.

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**The Skill Factor in Politics: Repealing the Mental Commitment Laws in California.** By Eugene Bardach. (Berkeley, Calif.: University of California Press, 1972. Pp. 291. $10.00.)

Thorough, literate, and sensitive treatments of complex issues are rare in this the day of the instant "nonbook." *The Skill Factor in Politics* is an excellent work deserving of widespread attention and serious reflection.

Bardach, a member of Berkeley's Graduate School of Public Policy, develops two broad themes simultaneously. One, captured in the title, examines in careful detail the attributes of political expertise needed to see a new policy through the legislative process. The second, noted in the subtitle, *Repealing the Mental Commitment Laws in California,* confronts termination—a little-understood aspect of the overall policy process—in the context of an emerging issue of great moment for citizens in many locales. While Bardach masterfully weaves his themes into a tapestry of beauty and impact, it is useful to separate them for review purposes.

The context is set in the book's first part by way of detailing the "attentive public" in terms of its concern for and stake in mental health policies and programs in California during the mid-1960s. The California setting is thoroughly characterized and appropriate linkages to longer-term trends in mental health practice in the encompassing environment are made. Several major fundamental issues, including prevalent, representative points of view on each, are considered: What is mental illness? Who is the "patient"? What are the causes of mental illness? What treatment methods exist? What are the rights and duties of those providing and receiving treatment? What are the trade-offs in terms of the quantity and quality of care? What institutional models are available to best meet service needs? Not surprisingly, the attentive public did not neatly sort itself out and line up into simple "traditional" and "progressive," or other dichotomized summary categories with respect to these issues. Clarifying and disentangling, "mapping" in Bardach's terms, the factions, interests, and alliances created in response to the major issues became on-going operational problems for the political entrepreneurs interested in making changes—problems whose complexity is ably captured in Bardach's ex post facto reconstruction and analysis of the "map."

The book's second part contains five mental health policies as seen through the eyes of the attentive public and the political entrepreneurs most interested in changing those policies. The civil rights of the mental patient are routinely threatened and abrogated by the "simple" commitment act. Bardach translated abstractions such as the denial of human rights and dignity into understandable operational terms with sensitivity and in great detail. Human rights aside, the eventual outcome is shown to rest far more on the somewhat bizarre interplay of key legislative personalities than on any grandiose concern for absolutist ethical imperatives. Four other policy changes related to budgeting, professional licensing, the structuring and functioning of state and local mental health service provision, and realigning bureaucracies at the state level are similarly treated.

"Entrepreneurship in Practice," the third and most creative part of the book, analyzes the political attributes of the key players and the main processes by which critical mental health policies were changed.

A "good" player strives to expend his political resources—analytic, marketing, and managerial—so as to capture the authorities and to neutralize his opponents, and to do so efficiently and with due regard for the timing of events in the unfolding context. "Skill" is simply, but convincingly, defined in terms of the political entrepreneur's efficiency and inventiveness in expending his resources. For instance, "the critical resources needed for marketing a proposal are those that facilitate rapid and efficient communication with individuals and groups both inside and outside the attentive public" (p. 218).

The concept of information-as-resource is stressed in this treatment, and in a policy arena where most regard the issues to be merely "technical and administrative," the critical, catalytic role of the expert in generating, analyzing, and presenting this information is heightened. Indeed, the role played by several consultants throughout the process can only be considered decisive—as is increasingly the case with many policy issues that have highly technical and complex aspects. In this case, the "essential entrepreneurs" were able to capitalize on the slight and piecemeal understanding of the context held by those in the attentive public: "pluralistic ignorance," in Bardach's terms. Understandably, legal specialists were mainly and narrowly concerned about the legal aspects of the various policy proposals; medical specialists had little appreciation of legal details and focused on alternative treatment methods to the near exclusion of all else; and most politicians, i.e., the "average legislator," did not know or care much about anything having to do with mental health policies at all. (Bardach could find only four senators and at best two assemblymen who "knew enough to care.") In this setting, lawyers were encouraged to concentrate on and support changes in treatment practices, doctors were made to focus on changes in the law, and politicians were "sold" in any way possible to get their support. Watching and documenting the players' responses as they began to realize and react to the possible indulgences and deprivations flowing from various proposed alternatives is a fascinating lesson in compromise, evolution, and bargaining —in short, a rich lesson in real politics as seen through the eyes of a careful observer, not the pinched view provided through the bunghole of a sterile theorist's 2 × 2 matrix or 2- or 3-variable regression equation.

Termination, the book's second theme, is evident throughout. Termination refers to the adjustment of policies and programs that have become dysfunctional, redundant, outmoded, unnecessary, and so forth. We do not know much about termination, although movements to "deinstitutionalize" and "debureaucratize," such as those captured in this book, are drawing attention to this neglected phase of the overall policy process. *The Skill Factor* provides the reader with a clear view of some of the difficulties one may encounter in attempting to terminate. Some of these difficulties are suggested in answering questions of the following sort: Who will suffer (benefit) from the termination? What kinds of compensation must be considered—no compensation, small compensation, equivalent, premiums? When should compensation be made in terms of what has been principally lost, e.g., wealth for wealth in the case of hospital attendents who stood to lose their jobs? When should it be in alternative values, e.g., compensatory respect for those professionals denied treatment "rights" in the licensing case? What can be learned from termination in a given setting that might inform and improve the initiation of policies and programs in the same or related fields and settings? For instance, closing down an antiquated and inhumane mental hospital may generate different, albeit equally inhumane, practices for those souls who need and are unable to receive services where they are eventually relocated. The list of pertinent questions is long, but seldom asked and less frequently answered. Bardach provides us with the raw materials needed to do both.

*The Skill Factor* has its inevitable shortcomings, but they are few and detract imperceptibly from its many strengths. Most weaknesses follow from the fact that the cases considered are extraordinary "success stories." In most settings, one would expect the entrepreneurs to make many more mistakes, to be less motivated, and to have less luck. No would-be entrepreneur should expect to use this work mindlessly as a cookbook; no neophyte analyst should delude himself into thinking that the same events have occurred in other, even closely related, settings.

A convincing argument, in my view, could be made that the principals in this little drama were just plain lucky.

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**Escape From Anarchy.** By Alan Bent. (Memphis, Tenn.: Memphis State University Press, 1972. Pp. 196. $6.95.)

This book attempts to use systems models and cybernetic analysis to outline a planning process rationalizing the urban intergovernmental system. Specifically, ". . . it believes that rational inputs into a rational decision making system should result in rational policies . . ." (from the introduction by George Blair). Planning, apparently physical, social and governmental, is seen as the linkage between the political system and the urban system.

The introduction outlines a cybernetics theory which ". . . applies the theoretical framework of systems models and cybernetics analysis in building an intersystem linkage between the urban system and the political system . . ." (p. 6). The first chapter elaborates, based on Buckley and Deutsch, a systems/cybernetics model.

Chapter II is a routine listing of major "urban discontents" emphasizing population and pollution. Chapter III lists the options for metropolitan government unification and success to date. Chapter IV lists the "goal seeking outputs" of the federal government to date, which have mainly been limited to grants-in-aids. Chapter V argues for vast, centralized "systems" planning, to ". . . transform amorphous agglomerations, that are typical of urban areas, into cohesive, unified organic wholes . . ." (p. 121); and Chapter VI considers existing regional planning agencies. Chapters II through VI, however, do not make use of the cybernetics model outlined in Chapter I.

Chapter VII is the heart of the book, containing Professor Bent's proposals for governmental reorganization, presumably based upon his systems model. It outlines the structure of an intergovernmental system, cybernetically linked by planning and government management. Highly centralized, the five levels are federal, federal region, state, regional council, and locality. In a rigid hierarchical setting, the Office of Management and Budget, with regional directors, serves as the nerve center. The levels apparently are to serve both as goal-setting devices and as part of the feedback system. The regional director is to have extension discretion in disbursing funds, since fiscal resources are the control mechanism by which the ". . . linkage-centers effect control over the actions and performance of their charges" (p. 177).

Some will regard this as an important work, since it attempts to apply concepts such as cybernetics to the planning process. As a theoretical work, in laying out certain issues, it may be a step in this direction. Those interested in applying systems models to physical planning or government planning (the book does not always separate them) should read it.

The book has, however, some substantial shortcomings. First, the traditional nature of the final proposal could easily be based on the "democracy, economy, and efficiency" reformer ideals for centralization in vogue during the early part of the century. They do not seem to rise out of any knowledge or insight gained by the use of cybernetics.

This objection leads to the second shortcoming. The parameters and specifics of the cybernetics model are never spelled out. Bent fails to specify exactly what is part of the cybernetics system, the feedback loops, or any "black-box" processes. This omission is troublesome even given the abstract nature of system theory. The model is rarely related to the author's description of the planning process. Where it is, as in the last chapter, it is distressingly vague or excruciatingly tantalizing to those who are interested deeply in planning or cybernetics or both.

Finally, the proposals are politically naive and often seem superficial. Short shrift is given to the advantages of the present decentralized system: the author merely states his preference for a more centralized, rational system. The work does not cite any of the "political economy" or "public choice" literature which argues that a market system of many competing governmental actors operates more efficiently and democratically than a centralized bureaucracy. Works by Ostrom and others are not cited. Centralized hierarchical rationality is sought, without noting local political values, federal level bureaucratic inefficiencies, failures of planning to date, or difficulties in implementing the model.

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**Private Pressure on Public Law: The Legal Career of Justice Thurgood Marshall.** By Randall W. Bland. (Port Washington, N.Y.: Kennikat Press, Inc., 1973. Pp. 206. $9.95, cloth; $3.95, paper.)

As the author indicates, this is not a biography but an analysis of the legal battles of

Thurgood Marshall, perhaps the foremost civil rights lawyer of our history. The coverage is of the cases and controversies in which Marshall has participated as an advocate for the NAACP, as Solicitor General of the United States, as a judge of the court of appeals, and now as an Associate Justice of the Supreme Court.

The treatment is "cool." The author outlines the issues and provides the background for understanding the strategy of the advocate as well as the decisions of the judge. From the study emerges a picture of a courageous, persistent, and ideologically consistent champion of civil rights who, although recognizing the differences between the roles of advocate and judge, remains steadfast in his commitment to the principle of equal justice. Professor Bland concludes that Marshall's accomplishments as an advocate outshine those as a jurist, although conceding that it is too early to make a definitive evaluation of his contributions from the bench. A revised doctoral dissertation, and still bearing some of the marks of such a product, the book provides a most useful review of the civil rights movement at a time when it was directed at destroying the legal edifice of segregation and in providing juridical protections for the victims of discrimination. (As late as 1919, as Bland points out, 12 blacks were sentenced to death, 75 to long prison terms, after a forty-five minute trial for their participation in a "riot" in which 200 blacks but no whites were killed.)

There is always the danger of our reading history backwards and of accepting what has been accomplished as the inevitable working out of economic, social, cultural, political and similar "historical forces." There are those who denigrate the works of men like Marshall and his companions as being ineffective. True, the abolition of forced segregation did not make civil rights champions out of most white Americans, and the securing of equal justice does not by itself ensure that blacks will secure their fair share of the economic funds of this world. It is also true that to win a favorable Supreme Court decision against a discriminatory practice is not the same thing as having that practice abolished throughout the length and breadth of the land.

On the other side, we should not underestimate the advantage of having the courts on the side of equality rather than on that of those who wish to impose racial segregation. We should not minimize the courage and contributions of Marshall and his colleagues. With the help of a handful of dedicated black attorneys and white liberal allies, and a few courageous plaintiffs and some scared defendants, they risked their lives and careers in a life long effort in behalf of the poor and the dispossessed long before it was the fashionable thing to do. They were the human instruments of the converging economic, political, and social forces, but in every day terms they took on the "power structures." They forged the legal doctrines. They used the courtroom to arouse gradually the conscience of court and country to provide the legal openings through which the modern civil rights movement came marching through.

Marshall is a champion of civil rights. He is a man of the law. He sees these as one and the same thing. For his conviction, shared by many, is that the forces of the law, the arguments of reason, and the structures of democracy are more apt to protect the rights of unpopular minorities, racial and otherwise, than is the resort to extralegal tactics. There is much in this history of the pre 1960's civil rights movement as described in this volume to support Marshall's position. Bland reminds us by the story of Autherine Lucy, an early victim of "student unrest," that the young are not always on the side of the poor and the black; by names like Governor Orval Faubus, that civil disobedience can be the tactics of the bad guys as well as the good ones. For it was Faubus who claimed that when the law was immoral, as he insisted was true of the Supreme Court's decision in *Brown* vs. *Board of Education,* that the proper recourse for the "decent" people was to take to the streets. By massing themselves in front of the schools and shouting abuses at the black children, Faubus insisted, these citizens were merely engaging in noncoercive protests. It was the white segregationists who insisted that under no circumstances should the police or the military ever be allowed to enter the school building or a college campus in order to protect the rights of black students to attend. We are also reminded by this history that the most persuasive and effective appeal used to secure eventual compliance with the civil rights decisions was to law and order—"you may not be in favor of integration, but it is now the law of the land."

Thurgood Marshall for decades has been in the forefront of the never ending legal battles to secure for all persons, and most especially for blacks, the rights promised by the Declaration of Independence and proclaimed in the Constitution. It is good to have this recording of his—and the nation's—accomplishments. We should not need to be reminded of how far we still have to go.

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**Politics and the Administration of Justice.** By George F. Cole. (Beverly Hills, Calif.: Sage Publications, 1973. Pp. 234. $10.95.)

One of the most complex areas of the Political System is the legal system. The myths surrounding the system, and its complicated rules, make it a very intricate field of politics. This complexity is analyzed by Professor George Cole, using the tools of role theory and administrative behavior. His primary goal is to "examine the administration of criminal justice within the context of the local political system" (p. 19). He does this by examining the overall administration of justice, relying on models of the criminal justice system presented by Herbert Parker, James Klonoski, Robert Mendelsohn, and others; then showing how these models have been used to explain the criminal justice system.

After reviewing some of the organizational and role-theory literature, relying for the most part on Talcott Parsons and Robert Merton, Professor Cole states that his approach entails an analysis of the "exchange relationship" of actors within the criminal justice system, and the bargaining of these actors to produce "criminal justice." "With a bargaining rather than an adversary model as a basic assumption, the study emphasizes the connections among law, politics, and administration" (p. 47). Cole accomplishes this by grouping the major actors within the criminal justice system—police, prosecutor, defense attorney, and court—and giving a detailed analysis of the functions played by each actor in the production of criminal justice.

Professor Cole examines each set of actors within the criminal justice system in a very logical manner. He begins by discussing police functions, and their relationship to the community. Next he treats the role of the prosecutor and his relationship to the other actors within the criminal justice system. This is followed by an examination of the defense attorney's role in this process. Finally, the court and its actors (clerks, judges and jurors) are thoroughly examined. Throughout his analysis, Cole reminds us of the relationship of each actor to the other, and to the general public.

The most interesting feature of this volume is Professor Cole's emphasis on bargaining and the "exchange relationship" between the actors within the criminal justice system, as his explanation of the plea-bargaining system which involves the prosecutor, the defense attorney, and the court. He also explains the bargaining power of the police in terms of the prosecutor's reliance upon them for "raw material" as evidence for prosecution, and their referring clients to defense attorneys who cooperate with them. "A police captain is probably going to be less likely to hand the attorney's business card to a prisoner if, on the basis of past experience, he has found that the lawyer is not cooperative" (p. 177).

Professor Cole concludes with a brief discussion of "due process in an administrative system" of criminal justice, and the "dilemma" of the "need for order and the preservation of civil liberties" (p. 226). Here he calls our attention to the Warren Court's revolution in civil liberties, and the changes brought on with the inception of the Nixon administration and his Court appointees. Cole asserts in the end that criminal justice reflects the "dominant values" of the larger political system.

This is a valuable volume for anyone wanting to understand some of the complex features of the criminal justice system. It gives a thorough review of much of the current criminal justice literature. It fails, however, to suggest any alternatives to the criminal justice system, and especially to some of its actors. Cole tells us how the present system could be strengthened, but he does not sufficiently demonstrate a need for the present system. What would be the conditions within a community without a police force? Or a court or prosecutor? These are some of the questions that must confront those of us interested in studying the criminal justice system.

Nolan E. Jones

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**The Truman Doctrine and the Origins of McCarthyism: Foreign Policy, Domestic Politics, and Internal Security, 1946–1948.** By Richard M. Freeland. (New York: Knopf, 1972. Pp. xii, 419. $10.00.)

**Seeds of Repression: Harry S. Truman and the Origins of McCarthyism.** by Athan Theoharis. (Chicago: Quadrangle Books, 1971. Pp. 238. $6.95.)

Revisionism, at its best, challenges received wisdom, forces reappraisal of things always accepted, adds new knowledge, and advances understanding. Its victories in the soft science of politics have not been less in their meaning for man than those in the hard science of physics. The revolutionary thought that governments exist for the governed and that it is not the other way around is not less formidable than the realization by some Greek that it was not the trees that stirred the wind but the wind that stirred the trees, nor the Copernican conception that put the sun and not the earth at the center of the solar system. But revisionism in history writing has a way less of establishing new truth about what was, than of ratifying today's predilection with yesterday's evidence, reconsidered. It is in this mode that the books by Professors Theoharis and Freeland are written.

The assumption of both the books is that President Truman and his principal surrogates were responsible for the phenomenon of McCarthyism. Both books go even farther than this and say that there were no substantial differences between Truman and McCarthy. As Theoharis puts the proposition, "The Senator and the administration differed not so much over ends as over means and emphasis" (p. vii). As Freeland puts it, "The practices of McCarthyism were Truman's practices in cruder hands, just as the language of McCarthyism was Truman's language, in less well-meaning voices" (p. 360).

It is assumed by both that America started the Cold War. For Theoharis, it began when Truman, immediately upon taking office, rejected what Theoharis says was Roosevelt's approach to foreign affairs as represented by the Yalta agreements—willingness to negotiate in good faith with the Soviet Union, his belief that Soviet promises could be trusted, the consent to compromise—and set forth instead upon a policy of containment and superior military strength (p. 70). "In contrast to Roosevelt's sophisticated international approach," Truman's policy stressed the primacy of power in international relations, and was "shaped by his World War I experience, his active participation in the American Legion, and his antipathy to the isolationism and antimilitarism of the 1930's" (p. 31).

The Freeland thesis seems to be that before Yalta there was "deepening tension" between the two powers as war came to a close. By February 1945, the Soviet intention to dominate Eastern Europe was becoming clear (p. 39). It was not a trust in idealism that led Roosevelt to praise the Soviet Union at the time of Yalta but rather a propaganda strategy designed to promote a policy of American international commitments through membership in the United Nations and economic aid. So, Roosevelt hid his distrust and anxiety over the Soviet Union and did nothing but praise it, especially after Yalta (p. 41). Truman continued the propaganda throughout 1945, concealed the tensions with the Soviet Union, and like Roosevelt, raised false hopes about both the UN and the Soviet Union (pp. 43–45). But the economic levers that were supposed to have been the principal instruments of postwar policy for both Roosevelt and Truman had no effect upon Soviet policy in Eastern Europe. By the beginning of 1946, Secretary of State Byrnes gave up the effort to use them, and the policy of containment began. The Truman Doctrine was the "first official American renunciation of U.S.-Soviet cooperation" (p. 168).

To support the proposition that Truman begat McCarthy, both authors stress the Truman loyalty program in 1946–47, the announcement of the Truman Doctrine in April 1947, the activity of the Department of Justice in the enforcement of internal security, and the Marshall Plan. What Theoharis and Freeland both show is that Truman used a hard anti-Communist line to get the Congress to approve aid to Greece and Turkey in 1947 and to enact the European Recovery Act in 1948. The record certainly indicates that he did do just that. Still, one can admit that Truman used hot rhetoric to advance his policies and yet feel that it was something more profound than Truman's hyperboles that filled McCarthy's sails and sent him on his short voyage into infamy. But the authors find Truman's rhetoric not only hyperbolic but diabolic, full of craftiness and design. The hard line against the Soviet Union required a hard line against suspected subversives at home, and the hard line against suspected subversives at home helped to promote the hard line against the Soviet Union, and thus it secured the passage of aid to Greece and Turkey and the European Recovery Program which was presumably intended to benefit commercial and other American interests. This seems to be the essence of the Freeland thesis, with which Theoharis is in general accord.

Freeland says more than once that it was only nine days after the Truman Doctrine was announced that the Federal Employee Loyalty Program was launched. He acknowledges that a case can be made for the view that the Federal Employee Loyalty Program was really intended to head off a tough assault on the Executive that had been building since the Dies Committee was created in 1938—an assault which had been frustrated by Roosevelt and which could hardly be stemmed when the 80th Congress turned up Republican in both houses (pp. 123–127) after the election of November 1946. But Freeland does not believe it. If Republican politics were the only reason, he speculates, Truman would have been defiant! Because Truman was not defiant, it must mean then that the Administration's foreign policy "may well have been" the inspiration for the Federal Employee Loyalty Program, and not the 1946 elections (p. 128).

One difficulty with this argument is that it seems to assume that had there been no Truman Doctrine there would have been no Federal Employee Loyalty Program. But Freeland also says at one point that the Administration was unable to resist the congressional Republicans on internal security (p. 201, 202). It can reasonably be asked then whether, in the absence of the Truman Doctrine or the Marshall Plan, congressional Republicans would have been passive, their sense of danger dormant, their energies unstirred. The evidence is to the contrary. The evidence is that with a majority in both houses for the first time in over a quarter of a century, shut out from the White House in four successive presidential elections, uneasy over the toleration of radical groups allowed by Roosevelt, bent on equating the New Deal with un-Americanism, the Republicans and their Democratic allies in the 80th Congress would have forced even more painful measures than the Administration used. An example is the McCarran Act with its provision for detention camps that Truman vetoed, only to have the measure passed over his veto.

According to Freeland, Truman produced a war scare in 1948 in order to get Congress to enact the European Recovery Program. Now it is true, he says, that the Soviet Union did impose a unilateral military accord on Finland and that the Communists did take over Czechoslovakia, but he thinks that the Czech coup was of no importance because the Soviet Union had dominated the country all along, and besides it was in the natural sphere of influence of the Soviet Union; and, he feels, the Finnish accord changed nothing (p. 278). The war scare was manufactured to get ERP through Congress and used to help Truman's chances of re-election (pp. 280 and 299). Theoharis does not go so far as to say that the "war scare" was manufactured by Truman for ulterior reasons but is content to say that Truman used the coup in Czechoslovakia, Soviet pressure on the Finnish government, civil war in Greece, and the chance of a Communist electoral victory in Italy to support a tough foreign policy critical of the Soviet Union (p. 54). Perhaps some would not find it unreasonable even now to think that there was much in Soviet foreign policy in 1948 to be critical about.

Ultimately, it seems to me, both these books fail in their purpose. Their common theme is that Truman produced McCarthy, but they do not demonstrate that he did so. What they demonstrate is that Truman had a difficult time with a reactionary Congress and a competitive Soviet Union and that he sometimes used the second to overcome the resistance of the first. The notion that the congressional dogs were sleeping on the issue of internal security until Truman aroused them is just contrary to fact. The assumption that the World War I veteran member of the American Legion flew like a hawk to tear the throat out of the dove of peace is an unwarranted piece of psychologizing.

Both books impose a greater degree of rationality and conscious manipulation on events than is allowable by the record, which shows that the Executive and the Congress were divided and hostile; that some of the executive agencies like State, Commerce, and Justice were either pushing their own policies independently of the president and of each other (like State and Commerce while Wallace was head of the latter), or that they were split within (like Justice and the FBI under Hoover); that party identification provided no unity since the Southern Democrats and the Republicans voted with each other; or that whatever unity of effort the war had provided disappeared when the war was over, both at home and abroad. The three years after the death of Roosevelt were the aftermath of war with all reins now tangled and slack, the horses spent, trembling and sweating, momentum lost, while a new driver was suddenly pitched into the driver's seat without experience at the reins or even knowledge of the road. It is just possible that the really hot story about these years is not that Truman managed to get foreign aid through the Congress but that he was not impeached; but then, perhaps, there was no need for such drastic action since it was expected widely that there would be a change of drivers in November 1948.

Both accounts fail to give Truman and the administration proper credit for the actions against reactionaries that he did take. He was, to be sure, not one of the leading libertarian presidents (how many have there been?), but he was better than most. He did hold off the ravenous reactionary coalition in the Congress for two years—from the time of his accession to office until the Federal Employee Loyalty Program—and he introduced that measure only after he had lost both houses of Congress. He and his Attorney General made sure that the Mundt-Nixon Communist Act was buried in the Senate Judiciary Committee and never reported out. As was said above, he vetoed the McCarran Act of 1950. He defended Dr. Edward Condon who was under attack by J. Parnell Thomas, Chairman of HUAC, as "the weakest link in America's atomic security." He fought the HUAC on Bentley and Chambers, calling the committee's investigations a "red herring." After the election of 1948, Truman and Attorney-General Clark began a determined effort to have the HUAC abolished. Although failing in this, they succeeded in blocking the reappointment to the committee of John Rankin and F. Edward Hebert, who had given Southern comfort to the Republicans, and had the moderate John Wood of Georgia appointed as chairman. In the four years of his term after the period covered by Freeland, Truman threw his support behind the Tydings Committee which tried to discredit McCarthy's attack on the State Department. He faced down McCarthy on the matter of Philip K. Jessup, and when the latter's formal appointment to the Sixth General Assembly of the UN was blocked in the Senate, Truman gave him a recess appointment instead. For the authors, these acts have some weight, but too little to preponderate.

The books fail to establish Truman as the author of McCarthyism for reasons special to each—Theoharis is too broad, and Freeland is too narrow. Theoharis often refers to people he calls "conservatives" or "McCarthyites," terms that cover all those disposed to take a hard line on the Soviet Union and on internal security, but such people existed before Truman, as he himself says (pp. 14 and 15); they had a base independent of McCarthy's; and they used him in their insistent assault upon the New Deal. The "McCarthyites" therefore existed before McCarthy, were in the Congress before he arrived, and remained there after his condemnation by the Senate. In view of this, it is hard to see how Truman could have created McCarthy or McCarthyism.

Freeland on the other hand proves too little in general, and nothing in particular of the substance promised by his title, the "Origins of McCarthyism." He concentrates upon two years, 1947 and 1948, in working changes on his principal thesis that the Truman Doctrine was invented to get aid for Greece and Turkey through an isolationist and reluctant Congress and that he had to go after domestic "subversives" to make the Truman Doctrine credible: if we are chasing the Reds abroad, how can we ignore them at home? And this is what led to McCarthyism. He does not discuss McCarthyism at all and never even defines it except by indirection—i.e., the anti-Communist emotions of the 1950s carried to excess (p. 4). But without ever examining the phenomenon of McCarthyism, he asserts that Truman and McCarthy were more or less the same in speech and practice. His book is not really about the origins of McCarthyism at all. It is about some aspects of the politics of two principal measures—aid to Greece and Turkey and the European Recovery Program.

Both writers reject sociological, psychological, ethnic, religious, geographical, and metaphysical interpretations of McCarthyism as incomplete, at least. The most probable interpretation, they say, is the political one. Having said so myself nine years ago, I am inclined to agree. But politics did not begin in 1947 and end in 1948, and Freeland's book needs to display a broader and deeper understanding of American political movements than it does if the two years he examines are to be put in proper perspective. As for Theoharis, he knows in some inchoate way that the anti-radical excitements of the Truman years were part of something more deeply basic in the American political and economic system than this or that piece of legislation. But some of his generalizations are unacceptable on their face. He says, for example, that "Until the 1920's . . . popular anti-radicalism had not been essentially anti-communist in character" (p. 6). Since there was no Communist movement of any consequence in America before then, this is hardly surprising. The best chapter in Theoharis (and it is a good one) is the one on Yalta (Chapter 4) which was evidently his doctoral thesis.

Both the books give less account than they should to the international moves of the Soviet Union and Freeland especially neglects this obligation. He says that he is not writing a diplomatic history and that he makes no serious attempt (that such a history would require) at analysis of Soviet intentions and policies (p. 12). He is concentrating, he says, on the way international developments were perceived and articulated by various groups in the United States. To leave out evaluation of Soviet foreign policy and to concentrate only upon American perceptions of that policy is to make it appear that the American responses were probably invalid, or at least explicable only in some term that leaves out half the equation, and is therefore incomplete.

To return to an earlier figure—the authors would like to prove that Truman was the wind that stirred motion in some dendrological McCarthy. It is possible to conclude, on their own evidence, that the same wind stirred both the President and the Senator, and that when it had blown itself out, McCarthy was down on the ground, and Truman was still growing.

EARL LATHAM

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**To Live on This Earth.** By Estelle Fuchs and Robert Havighurst. (Garden City, New York: Doubleday and Company, 1972. Pp. xiii, 390. $8.95.)

**One Hundred Million Acres.** By Kirke Kickingbird and Karen Ducheneaux. (New York: Macmillan Publishing Company, Inc., 1973. Pp. xv, 240. $6.95.)

The rediscovery of the American Indian is a phenomenon of the late 1960s. The publishing industry, not slow to capitalize on a new marketable resource, has issued innumerable anthologies of immortal Indian prose and poetry, occasionally illustrated by photographs of vanished days of glory. These efforts are marked by two common characteristics: non-Indian authorship, and a singular lack of appreciation for the highly politicized context of contemporary Indian life. Perhaps more crucially, they totally ignore the strong commitment of Indian people in the United States to the continuing struggle for self-determination and cultural autonomy. As an Indian I consider most recent "Indian books" personally offensive and socially destructive, for they perpetuate the dangerous myth of the inevitable disappearance of a distinct Indian community in the modern world, and they reinforce the myth of the preeminent expertise of the non-Indian observer/commentator. As a social scientist I consider the bulk of this current "scholarship" both intellectually parochial and politically naive. Given the overwhelmingly political nature of Indian policy in the United States, it is especially unfortunate that political science has largely followed the tradition of relegating the investigation of Indian affairs to anthropology, where Indian communities have remained trapped in the never-never land of the ethnological present. The fact that the *American Political Science Review* is now turning its attention to the review of Indian-oriented materials is a promising sign that this state of affairs is beginning to change, to the mutual benefit of the scholarly and Indian communities.

*To Live on This Earth* and *One Hundred Million Acres* are both essentially sources of solid reference material; both draw attention to critical dimensions of the American Indian community; both hint at the complexity and importance of political analysis of Indian status. This, when added to their general conscientiousness and seriousness of purpose, is enough to draw favorable attention from the Indian community, and among scholars interested in Indian affairs. For political scientists, however, the message is slightly different. These books point toward new and exciting areas of political research, reaching far beyond the issue of Indian status alone. Indian affairs have been ignored altogether too long in the emergence of an extensive literature on political development, political economy, ethnicity in politics, political socialization processes, etc. I hope that these two books will stimulate such research, suggest fruitful new directions for research, and support attempts to generate theory capable of dealing broadly with the dimensions of political mobilization in tribal, minority, and industrial communities.

*To Live on This Earth* is one of the few systematic and wide-ranging scholarly attempts to develop and test basic hypotheses about Indian education in the United States. Professors Fuchs and Havighurst examine the several types of schools serving Indian clientele, and investigate such vital policy areas as Indian attitudes toward schools, teachers, curricula, etc. They also examine such generally held notions as the existence of pervasive low self-esteem among Indian students (which they deny), cultural insensitivity among teachers and administrative personnel (which they partially substantiate), etc. In large part, the book is a straightforward report on research; at the end, however, it proposes certain approaches to the development of Indian educational policy which grow out of its findings. A particularly useful inclusion is the appendix, which gives details of the survey schedule used, the nature of staff composition, and so on. All in all, this study is an unusually conscientious approach to Indian research.

This very conscientiousness, however, in a sense camouflages the basic weakness of the book from the point of view of Indian and politically oriented scholars. Ordinarily, it must be admitted, political scientists tend to view education per se as peripheral to their primary interests. Yet education is a primary agent in political socialization and acculturation; it may have decisive influence on the very process of individual identity formation. Indian education, then, is central to the analysis of the political status of Indian affairs as it reflects the interests and goals of the dominant community with regard to Indians. In addition to its political content, Indian education has been administratively shaped in the explicitly political environment of Washington, D.C., so it is doubly political. Since the political nature of education applies in all communities, it is especially unfortunate that the authors of *To Live on This Eearth* give so little attention to these aspects of educational policy and practise.

The book also fails to confront the issue of the general political and cultural status of

American Indian society today, and of the future of that society. The authors in fact eschew any intention to take up this issue, stating that such decisions must be made within the Indian community. I agree. The thrust of their analysis, however, is to emphasize and reinforce the status quo, a situation in which political constraints prevent precisely such Indian self-determination. Thus, evidence from interviews with Indian students, parents, tribal officials, and others is cited in support of an emphasis on training students in skills relevant to integration with the dominant society, at least in the economic sphere. It is difficult to determine whether this attitude reflects an intrinsic positive evaluation of such integration and adaptation, or whether it represents resignation to the status quo. Urban, industrial, Western society (and social theory!) has hammered away at this theme of inevitable cultural amalgamation for so long and at such a critical level that isolated tribal communities are extremely vulnerable to the introduction of just such social objectives; indeed, that is precisely the intent of educational manipulation. Authors like Vine Deloria, Jr. (in *We Talk, You Listen*) argue that alternative social and political structures are possible for tribal communities and for industrial states, and that the tribal tradition may yet play a creative role in the emergence of these new forms. Whether or not one agrees with this position, it is important to emphasize the cost of locating analyses of minority and/or ethnic communities solely within static visions of the contemporary political world. Akzin, Enloe, Deutsch, Barth and others have called attention to the complexity of the ethnic issue, and social scientists ignore it at their peril.

Kickingbird and Ducheneaux's *100 Million Acres* suffers a similar failure to explore deeply the political dynamics behind the formulation of political policy affecting Indian people, although it does offer revealing anecdotes about the roles of political personalities in particular policy decisions. It is, however, in general a more significant book, albeit less "scholarly" in style. The two Indian authors, a lawyer and a journalist, consistently offer careful legal research into the status of the land rights of Indian communities, and, coming from "within" the Indian community, dare to tackle the introduction of a systematic positive program of action designed to lead to the political re-emergence of viable Indian communities. They see consolidation of the Indian land base as the vital first step in the development of realistically self-determining Indian politics.

From the point of view of theoretical interest, the work suffers from certain omissions. The political analysis is largely anecdotal and peripheral; the internal dynamics of decision making are referred to only in passing. Two kinds of work are needed here: First, there is a need for research on the activities and interests of political actors involved in the formation of Indian policy. Perhaps even more fundamental is the need for investigation of the whole area of interaction between "tribal" minorities (which exist, for example, in the USSR, China, India, Australia, as well as Africa and South America) and industrializing nation-states. Such studies often reveal as much about the politics of dominant systems as about the problems of the tribals themselves. As an early Commissioner of Indian Affairs once remarked, Indian Affairs is a bellwether of the health of the American political system. How fruitful then might a new political concern for Indian affairs become?

FRANCES SVENSSON

*The University of Michigan*

**Judge Learned Hand and the Role of the Federal Judiciary.** By Kathryn Griffith. (Norman: University of Oklahoma Press, 1973. Pp. xi, 251. $8.95.)

John P. Frank has said that "most expert judges of judges would choose the Court of Appeals for the Second Circuit . . . as the ablest court in the United States" (p. 24). From 1924 to 1951 Learned Hand was a member of that court, and his service earned him the reputation as one of the most distinguished jurists of the twentieth century. Judge Charles E. Wyzanski argued that Hand was "universally acknowledged as the greatest living judge in the English-speaking world" (p. 9). In spite of such acclaim, Hand was never elevated to the Supreme Court, although Presidents Harding and Franklin Roosevelt considered the possibility. The work of the Second Circuit, "Learned Hand's Court," in matters of constitutional interpretation, immigration and naturalization, patents, subversive activities, obscenity, and criminal procedure, provides ample opportunity to understand Hand's pervasive influence on American law.

As its title indicates, this book is principally concerned with Hand's conception of the judicial function. Profoundly influenced by his teacher, James Bradley Thayer, Hand falls squarely in the tradition of judicial restraint. "He thought that a judge must decide in accordance with decisions arrived at through the political process without attempting to impose his own values. When it seemed appropriate to him, he deferred to the higher court, to the legislature, to administrative tribunals, and to the spirit of the time" (p. 84). So thoroughgoing was Hand's deference to popular institutions that during his entire tenure he never voted to invalidate congressional legislation. The importance he attached to judicial limitations did not vary from issue to issue. Like Oliver Wendell Holmes, he argued against the use of substantive due process as a protection for economic and property rights. Through his intellectual association with Holmes, Harlan Fiske Stone, and Louis Brandeis before the "Constitutional Revolution of 1937," Hand's opinions were accorded the label of liberalism owing to his intense efforts for judicial humility. But when American appellate courts turned to issues of free expression and civil liberties after 1937, Hand's reverence for judicial restraint continued with iron-clad consistency. As Professor Griffith explains, his "disagreement with the most active judicial defenders of liberty was not with its necessity to human life but with the proper function of a judge in a democratic society. He . . . did not assume the role of social or moral reformer" (p. 41).

The "new activism" after 1937 was concerned with the judicial protection of civil liberties essential to the maintenance of free government. Its source was Stone's statement on "preferred freedoms" in *U.S.* v. *Carolene Products Co.* [304 U.S. 144 (1938)]. Just as Hand refused to accept the doctrine of substantive due process as constitutional support for private property, he did not approve the doctrine of preferred freedoms. With such opposition to the new activism, Hand assumed "what by modern standards is a very conservative view of the court's role" (p. 41). In the 1940s judicial restraint was synonymous with a rejection of claims of civil libertarians; during this decade, the judicial philosophies of Hand and Felix Frankfurter were congruent. Yet Frankfurter was willing not only to profess the virtues of judicial limitations in civil liberties cases: he often added to such opinions his personal predilections about the wisdom of a claim. Hand was unbending, more consistent in his devotion to judicial restraint and deference to popular institutions even than Frankfurter. As Griffith explains, however, within the contours of judicial limitation, Hand allowed himself some latitude for creativity. Judges "must remember the pragmatic origin of the law which they seek to interpret, and, while they must not go contrary to it and cannot legitimately go beyond it, they must be a party to its growth and development . . ." (p. 180).

Many will find the author's approach old-fashioned. For example, she startles the reader, especially the political scientist whose specialty is judicial behavior, by stating that "[f]ew students of the American judiciary would take issue with Alexander Hamilton's assertion that 'the judiciary . . . may truly be said to have neither *force* nor *will,* but merely judgment' " (p. 83). Legal realists have shattered the myths of judicial objectivity. Judges are important policy makers regardless of whether they adopt roles of judicial activism or restraint. Throughout the book Griffith's writing manifests an uncertainty about the policy making function. Her inclination, as the choice of Hamilton indicates, is to deny the importance of individual beliefs or will in judicial decision making. And, looking to the future, she says it is "increasingly imperative that judges be thoughtful and self-conscious about their role and refrain whenever possible from policymaking decision" (p. 164). Yet to assume that judges, even by adopting roles of passivity, can avoid policy decisions is an untenable position. Griffith herself is forced to reach such a conclusion as she discusses civil liberties: "Questions arising under the Bill of Rights invariably relate to policy and choice" (p. 134).

Had the author utilized some contemporary concepts in the analysis of judicial behavior, she might have avoided some of the difficult, and occasionally inconsistent, conclusions about judicial beliefs, will, function, and policy choice. As with Frankfurter, an analysis of Hand's court behavior requires an analytic division between personal attitudes or values and role definition. In the concluding chapter, Griffith begins to come to grips with such a distinction: "Learned Hand had a rare capacity to distinguish between his personal philosophy and his duties and responsibilities as a judge . . ." (p. 230). Such a distinction could well have been a more explicit theme throughout the book.

This book is limited to Hand's conception of the judicial function; its materials come almost exclusively from *The Spirit of Liberty,* a collection of Hand's essays published in 1952, and from court opinions. The author has made no attempt to analyze intra-court relations, personal correspondence and memoranda, and other primary source material often important in biography. In this regard, and because it eschews contemporary methods of analysis, the volume is far less significant than Marvin Schick's *Learned Hand's Court* (Baltimore: Johns Hopkins Press, 1970), published three years earlier.

ROBERT G. SEDDIG

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**To Be a Congressman. The Promise and the Power.** Edited by Sven Groennings and Jonathan P. Hawley. (Washington, D.C.: Acropolis Books Ltd., 1973. Pp. 258. $8.95.)

This book is a collection of eleven chapters on various aspects of the congressional process written by former congressional fellows. The topics covered include campaigning, constituency casework, informal clubs, committees and committee assignments, the seniority system, and leadership. The authors bring to their efforts extensive information and insight about Congress, and this is both the major strength and weakness of the book.

A number of the chapters reveal substantial sensitivity to the intricacies of Congress and present a surprisingly large amount of important information in a very readable fashion. But too many chapters take on the tone of insiders telling outsiders how Congress really works with much of the information presented being simply too specific and detailed and devoid of political significance to have much interest for the general reader. For example, the chapter on campaigns is replete with statements like "The headquarters should be attractive and reflect *esprit de corps*" (p. 11), and "A pleasant and cheerful voice symbolizes a friendly, interested, and optimistic candidate" (p. 11), and "Similarly, a banjo player or band can help attract an audience" (p. 16). In short, the campaign chapter reads like a cookbook recipe on how to get elected, with scant attention given to the role of elections in elite recruitment. Similarly, the chapter on freshman socialization devotes too much attention to a specific freshman and does not adequately address the more general problem of legislative adaptation.

The most successful chapters—in the sense of presenting basic information in a systematic way and drawing some implications about congressional behavior from this information—are those on seniority and committee leadership by Jonathan Hawley, leadership by Jean Torcom, informal clubs by Sven Groennings, and Congress and the press by Delmer Dunn. The Hawley chapter on seniority manages to present the standard arguments for and against seniority in a surprisingly interesting fashion and covers quite well recent developments in seniority reform in the House and Senate. The Torcom chapter on the leadership of Everett Dirksen provides an insightful addition to the growing body of literature on leadership styles. Likewise, Groennings's chapter on the House Wednesday Group adds to our knowledge about the functions and operation of informal legislative clubs. Finally, Delmer Dunn's contribution on Congress and the press raises some thought-provoking questions about the role of the press in agenda setting and the comparative use of various media by Representatives and Senators.

The remaining chapters are not as successful, often getting bogged down in unessential detail. Given the many readers available on congressional politics, I would argue that *To Be a Congressman* does not stack up very well for a number of reasons. First, there is no focus or organizing theme to the chapters beyond an attempt to present "an introduction to the practical aspects of congressional life" (p. viii). Second, numerous topics receive little if any coverage; for example, policymaking and legislative oversight are practically unmentioned. Third, the kind of information presented is generally so practical that any notion of Congress as a political institution operating in a political environment tends to be lost. The professional student of Congress will find useful a few of the chapters praised previously. But overall the book remains a disappointment for those hoping to gain important insights into the workings of Congress. As a minor point, the prospective user of the book should not rely on the book jacket as an indicator of the contents of the book. On the jacket are listed such potentially muckraking topics as "The little-known but effective 'Clubs' in Congress," "How the Kennedys win elections with the 'Kennedy Secretaries'" and "Does Congress have any ideas of its own?" The reader expecting to be titillated by such topics is in for a sharp disappointment.

HERBERT B. ASHER

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**Representative Government and Environmental Management.** By Edwin T. Haefele. (Baltimore: The Johns Hopkins University Press for Resources for the Future, 1973. Pp. xii, 188. $8.95.)

In his Foreword to this book, Joseph L. Fisher, president of Resources for the Future, says that it "takes us into new dimensions of problems we have long been dealing with along other lines" (p. v). This statement, I think, pretty clearly indicates the importance of the book. It moves Resources for the Future (and, one hopes, its next-door neighbor, Brookings) firmly into what we might call the "public choice" arena. In this book, Haefele not only examines the problems of environmental management but also, as the title says, representative government. The focus is on the design of institutions of government that have some reasonable probability of producing good decisions about the environment. Even those who do not agree with Haefele's argument in detail will agree with his stance and approach to the problem.

The bulk of the book is a set of mildly altered papers which Haefele has read at meetings or, in some cases, has published elsewhere. Like all such collections of papers, it suffers from a certain lack of focus and some repetition. These defects, however, do not greatly reduce the value of the book. Each problem in the book is discussed in a way that brings the political decision-making apparatus into the foreground of an environmental problem. As those who have been following Haefele's formal work in public choice will anticipate, there is a considerable emphasis on logrolling as a method of reaching suitable conclusions. Indeed, his mathematical logrolling model is presented here in greater detail than anywhere else I have seen it. Looked at from the standpoint of specialists in public choice, this mathematical model which permits investigation of various logrolling activities is the most interesting part of the book. At the practical level, perhaps his suggestion of the "general purpose representative," a representative for a given geographic area who serves on a number of different boards dealing with different subjects and having different geographic coverage, is perhaps the most interesting innovation.

Both those who have been working on environmental management and know little or nothing about the modern approach to politics *and* those who have been working on politics will find much in this book to think over carefully. I think that it would be most useful for graduate courses in environment and even for some advanced undergraduate courses.

GORDON TULLOCK

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**Black Civil Rights During the Johnson Administration.** By James C. Harvey. (Jackson: University and College Press of Mississippi, 1973. Pp. xiv, 245. $4.95.)

This account of the administration which saw the strongest commitment to equality for black Americans in our history offers a compact and useful summary of a historic period of change. Relying extensively on a wide variety of published materials, Professor Harvey gives a chronological account of the forging of the major statutes and summarizes some of the policy disputes and results of the enforcement process. The book makes extensive use of excerpts from contemporary accounts and speeches.

The study underlines the unique importance of the civil rights decisions of the Johnson period and makes one wonder why they have received so little serious attention from political scientists. The extraordinary use of federal power to transform some basic social and educational practices of a vast region of the country and to raise fundamental issues about our urban future has generated little scholarly inquiry. The experience is often dismissed on campus with a cliche, usually a false cliche. Perhaps this book will begin to stir interest in the research that needs to be done.

There are several reasons for the thin research base, I suppose, when civil rights is contrasted with a field like environmental protection. President Johnson is hardly a sympathetic figure for most academics, and it is often difficult for people mobilized against the Vietnam War to concede the elemental importance of President Johnson's civil rights record. In a discipline with little surviving tradition of public administration research, few political scientists have either the training or the interest necessary to study adequately the administrative enforcement process which has touched so many important aspects of American society. Finally, research has been hampered by ideological disarray. The emergence of the black power movement and the development from the simple issues of lunch-counter segregation and voting discrimination in the rural South to the very complex and emotional questions of school and housing segregation in northern metropolitan areas may have discouraged research. At any rate, Professor Harvey's broad search for data has turned up little significant work by political scientists.

While this book takes the Johnson contribution seriously, it is in no sense an exercise in hero worship. The narrative is laced with comments of contemporary civil rights critics. Each forward motion, of course, tended to reveal more basic difficulties. The book illustrates the difficulties of liberal officials attempting to cope with problems which simply would not yield to the incremental approaches so basic to the American political and administrative styles. Although the administration was achieving major breakthroughs, its best critics knew that the results would be flawed unless much more was done, sometimes more than the President had the power to do. Harvey moves increasingly toward identification with the critical view as the book progresses.

The principal asset of this book is also its weakness. Its comprehensive character and its effort to incorporate a wide diversity of sources makes it useful as an introduction to the period or a reference source more than as a definitive analysis. The author relies very heavily on secondary sources, often giving substantial weight to speculative political commentaries. The book does not build a unified interpretation, and it offers little discussion of the significance of the period for our general understanding of the political system. This lack of a perspective at times leads to an underestimation of the Johnson accomplishments.

At a time when the presidency is discredited and political cynicism is rampant, reviewing President Johnson's civil rights record is a refreshing and restorative experience. While it is true that President Johnson offered only tentative first steps toward resolving the intense segregation of our great cities or the crippling economic crisis of poor blacks, he did move decisively and effectively against particularly vicious forms of segregation through law in the South. Blacks vote and hold office in the Deep South and attend schools substantially more integrated than those in the North because of processes his administration set in motion and sustained in the face of enormous political pressure. LBJ knew that this record was his central gift to the country, and he knew that the achievement was incomplete. His last public appearance was devoted to a passionate appeal to the country to finish the social revolution he helped set in motion.

After President Johnson died, many whose lives were touched by his Administration paid tribute. As his body lay in state in the Capitol, people waited in a line a half mile long on East Capitol Street at 3:00 A.M. Many of those waiting were black parents holding the hands of small children. They were people who could remember a southern President telling Congress about his early experiences of discrimination and pledging that "We shall overcome." They waited to honor a man who shared with Martin Luther King much of the credit for the Second Reconstruction. This book will help the reader understand why they were there.

GARY ORFIELD

*Brookings Institution*

**Unwanted Mexican Americans in the Great Depression: Repatriation Pressures, 1929–1939.** By Abraham Hoffman. (Tucson: The University of Arizona Press, 1974. Pp. 207. $9.75, cloth; $4.75, paper.)

During the Great Depression, many Mexican nationals were repatriated from the United States. This bitterly controversial event has been almost completely neglected by scholars. Professor Hoffman's study is the first book-length account of it. For research sources he has relied heavily on original papers and documents from a variety of archives. The result is an eminently readable book containing a wealth of new information.

Before discussing repatriation itself, Hoffman surveys the movements of Mexicans to the United States before 1929. Pushed from Mexico by extreme poverty and political instability, these immigrants had little opportunity to bargain for economic and other rights in the United States. The fact that they were forced into the lowest rungs of American society aggravated pre-existing anti-Mexican stereotypes. According to Hoffman, widely held stereotyped assumptions about Mexicans were major components of the rationale for repatriation.

Considerable attention is devoted to the vigorous but unsuccessful interest-group effort of the 1920s to amend the immigration laws to rigidly limit Mexican immigation. According to Hoffman, the onset of the Depression markedly improved the political influence of restrictionist interest groups. The development of widespread unemployment made exclusionary arguments increasingly persuasive, and the national government came under massive pressure to expel illegal Mexicans. Hoffman correctly emphasizes anti-Mexican prejudice as an important source of this pressure, but that theme is so pervasively woven into the book that other possibly valid explanations for repatriation are underemphasized. For example, the reader of Hoffman's book will get little appreciation of the extent to which employers used illegal Mexicans to displace American workers, to fight unionization, and to freeze wages and working conditions. These considerations were important sources of repatriation pressures, and they were not always connected with prejudice against Mexicans.

Hoffman is critical of the federal government's repatriation role. He notes that government authorities had been indirectly responsible for the large accumulation of illegal Mexicans during the pre-depression years. In deference to politically influential employers, there had been little effort to eliminate the widespread and well-known use of illegal Mexican labor. The American government is portrayed as suddenly reversing itself and punishing Mexicans for doing what it had at least indirectly been encouraging them to do for years.

Immigration authorities staged "raids" from coast to coast. When apprehended, illegal Mexicans were given a choice between voluntary departure and deportation. Many American citizens of Mexican ancestry were also detained for questioning, and many American-born children (who were automatically American citizens) were sent to Mexico with their parents.

Important as the deportation drive was, Hoffman notes that it was not directly responsible for the exit of most returnees. Many left the United States simply because they had lost their jobs. Many others left under the prodding of state and local governments; this pressure commonly took the form of denying certain jobs or welfare benefits to non-citizens. Hoffman presents an extensive case study of the ambitious repatriation drive conducted by Los Angeles County. In addition to focusing on the evolution and operation of this program, he devotes considerable attention to clarifying its relationship to repatriation policies of the American government and to non-governmental drives sponsored by private interests in the Los Angeles area. Hoffman provides a good deal of new information in a chapter on the repatriation role of the Mexican government.

The title's suggestion that the book is simply about pressures to repatriate "Mexican Americans" is misleading. That concept commonly refers to American citizens of Mexican ancestry (and, except on p. xiii, Hoffman follows that usage). Although American-born children were sent to Mexico with their parents who had illegally entered the United States, the overriding concern of most repatriation interests was the departure of *illegal* Mexicans, and that is in fact the predominant concern of this book.

George C. Kiser

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**Inequality: A Reassessment of the Effect of Family and Schooling in America.** By Christopher Jencks et al. (New York: Basic Books, Inc., 1972. Pp. 399. $12.50.)

The tie between policy and school was once a traditional concern in the study of politics, but, for reasons now unclear, political science has been turned away from that connection for most of this century. Our thin interest explains why recent controversial education studies have lacked contributions by political scientists. These studies (the "Coleman Report" in 1966, U.S. Commission on Civil Rights' *Racial Isolation in the Public Schools* in 1967, Mosteller and Moynihan's *On Equality of Educational Opportunity* in 1972) reached a climax in late 1972 in the volume reviewed here. Of these analysts Moynihan is the only one with an understanding of formal political science, although he has not claimed this often or loudly in public places.

Yet this volume by Jencks, both in what it does and does not say, relates to our current emphasis upon policy outcomes. For this book challenges an explicit assumption of policy macroanalysis, namely, that policy can significantly affect the distribution and redistribution of social resources. Rather, *Inequality* claims that an ostensibly powerful institution—the school—has little independent influence upon citizens' "success," as measured by income or occupation. Indeed, the labyrinth of its tables and regression analyses, relating independent measures of school resources to school outcomes, seemingly suggests that "nothing affects anything," as Lester Thurow noted.

The press interpreted the results to mean that "schools made no difference" in improving life chances. While false in conclusion, this judgment precipitated political action. Taxpayer groups, already resisting the school costs in referenda contests, could find solace in a scholarly volume. In Washington, Moynihan, discouraged by his findings that the marginal benefits of using school investment for equalizing incomes were low, encouraged the Nixon Administration rather to consider the family assistance plan. That, too, however, failed for lack of will in the White House and opposition in Congress.

Nevertheless, the book became an instant source of criticism among diverse social scientists (but never among political scientists) because it touched so many fields. Yet these critics always found some element of analysis of conclusions with which they agreed. Partly that is because the book covers so many interactions of school and society that one is bound to find *some* agreement *some*where. Thus some conventional wisdom is confirmed, such as the high association between family status and academic achievement. The agreement also arises from the realization that school reforms of the 1960s did not come up to expectations, whether because of inadequate design (a familiar Moynihan criticism) or inadequate resources.

But this book is most contentious because it challenges scholarly notions that schooling can serve as a major vehicle of upward mobility, thus making for "success" in the American culture. Jencks finds, when the unit of analysis is the individual and not groups of students (as in the Coleman Report), that "schools serve primarily as selection and certification agencies, whose job is to measure and label people, and only secondarily as socialization agencies whose job is to change people. This implies that schools serve primarily to legitimize inequality, not to create it" (p. 135).

In this review, I will raise only a few points. First, in focusing upon success and certain measures of income inequality, how can the authors know that they have chosen the "correct" or, indeed, the only output which schools may influence? Or, why do they assert (without citation) that this or other social institutions serve—or should serve—*equalizing* functions in a democratic society?

On the question of outcomes, Jencks does note that there may be more important things than academic success achieved in school, such as happiness, the reduction of racial antagonisms, or other affect qualities. Indeed, they claim that school resources may be justifiable on these other accounts alone. Of course, the affect domain is harder to quantify and validate than the cognitive, but efforts which focus only on the cognitive effects of schooling do not give a full account, and possibly do not treat the most important aspect of education. In this volume, it is true, there is some coverage of school influences upon affect qualities, but it is remarkably skimpy. The defense that cognitive effect data are the most easily quantifiable is too thin when we recognize that major policy recommendations (some implicit and yanked out of these scholars' work by others) are being based upon these data. That is, such recommendations might not be supportable if we knew more about the positive influence of schools upon non-cognitive aspects such as attitudes and emotions.

Another assumption of this volume is that schools ought to be able to redistribute the social resource of "success" more equitably. But it is basic sociology that schools, like any institution, here and elsewhere, are conserving mechanisms, reinforcing the existing distribution of resources and values. Certainly political scientists are familiar with this distributional inertia in the prevalent findings of the small ratio of outputs to inputs in the political system, whether one measures municipal ordinances, judicial appeals, congressional actions, or even the ubiquitous school board actions.

Why then should we expect to find that the school system increases income for most individuals when that may well not be its purpose at all nor the reason most students attend? Schools, themselves political systems, have always distributed rewards and penalties. Plato explicitly set out a reallocative mechanism in *The Republic,* and while schools today may not contain the direct equivalents of his gold, silver, and brass, they still reallocate valuable resources. Some students get high grades, some make the student council or band, become cheer leaders or class presidents—but most do not. The failure to receive good grades has bred, as Lewis Dexter notes in *The Tyranny of Schooling,* a society-wide fear of being regarded as stupid, testifying to the reallocative —not equalizing—nature of that policy.

Further, because attendance is everywhere compulsory, we cannot assume that all students attend in order to "better" themselves by achieving subsequent "success." What they find is a miniature of society. One need not call for radical reconstruction or "deschooling" to understand that the schools continuously offer students a working model of inequality. Twelve years of participating in that inequality surely cannot condition many to expect much more in the postgraduation reality.

Besides questioning basic assumptions, I should also like to ask whether political scientists might not approach the same data in any way different from these authors? The book's paradigm relates characteristics of students and their schools to measures of economic success, but it is silent on the role of the political system, aside from some early and brief statistics on the variation among states and school districts in expenditures, without relating it to the major dependent variable. What is not conceptualized is the difference, if any, made by the political qualities of schools.

Part of the omission may be due to the authors finding that there is far more variation *within* schools systems than *between* them, as did Coleman. If so, a hasty inference might be that the political factors are independent of these intraschool variations. But the pattern of variation *within* schools is not identical from system to system, and so political qualities might be hypothesized as precedent factors shaping those intersystemic variations. As with the most recent macroanalysis, the political system may be found more influential in redistributive policy results than was thought a decade ago. Certainly redistribution analysis is needed to show what states do with the school funds allowed by their economic environments. Similarly, at a substate level we need to know what effects upon school outcomes and subsequent success are traceable to: the political climate of the classroom, school dependency upon the local government, the impact of the elections surrounding schools, group conflict over locally hot issues like desegregation, curriculum reform, taxation, and so on. These are questions not touched upon in *Inequality* or in much current research by political scientists.

Nor is much attention given in *Inequality* to the particular combination of public policies which *does* have an effect. The recent controversies over such school matters as race, unionization, student rights, community control, and finance are alike in creating inordinate waste because of the typical randomness by which Americans approach any reform. But, in the end, *some* policies are successful even though the bulk of them are not. Thus in matters of desegregation, Smith and Downs have shown workable methods of *Achieving Effective Desegregation* (1973), while studies of the U.S. Office of Education by Robert Crain and others, have described combinations in the South that depress racial polarization and increase achievement among students. But in the basically pessimistic mood in which *Inequality* proceeds, nothing seems to explain much of anything that is measurable, and hence the authors need to fall back upon the extraordinary nonexplanation of "luck" as the cause of "success."

Given the poverty of theory suggested by this nonexplanation, another strategy might be to employ grounded theory. This begins by examining policy alternatives which *do* seem to accomplish their program objectives. Such an approach, while lacking the elegance of inductive theory, seems far more preferable when induction only leads to speculating about "luck." In another era, we would be talking about "God's will," always an uncertain source of prediction.

*Inequality,* while important for those in the public arena of school policy, is also a contribution to the sociology of knowledge. Ultimately, it seeks to deal with the best available theory and methods to explain the interactions of individuals and institutions. Given all that, however, it is a highly sophisticated demonstration of how little we really know about that interaction. Many will not be as pessimistic about schools as the authors of this volume, because the institution provides a means for at least some citizens to improve their life chances, and possibly that is the most one can hope for. Jencks, in the book's conclusion, is dissatisfied with even this gain. The reform tradition of improving individual chances by "ingenious manipulations of marginal institutions like the schools" will not do, he argues, for "we will have to establish political control over the economic institutions that shape our society. This is what other countries usually call socialism. Anything less would end in the same disappointment as the reforms of the 1960s."

Despite the question raised here, I must emphasize that many of the insights and policy recommendations are very impressive and address a major social issue in an ambitious way. A book is important in social science not because its authors are "right" but rather because they try to deal with major questions of public life by using broad theory and sophisticated methods. In those terms this is surely an important book.

FREDERICK M. WIRT

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**The Knapp Commission Report on Police Corruption.** (New York: George Braziller, 1973. Pp. 283. $8.95, cloth; $3.95, paper.)

This is a narrow, specialized, practical book, a book which was not intended to raise general, theoretical issues, but which does so in spite of its own best efforts to the contrary. It is easy to say what this book was intended to do—namely, to describe, in great detail the pattern of police corruption in New York City and to prescribe some ways to reduce or eliminate it. It is, however, not so easy to grasp what it accomplishes along the way. Mixed in with the rather dry and often repetitive prose which seems characteristic of reports of this kind, one encounters interesting insights into the nature of public organizations in general, and police organization in particular, problems in the administration of justice and the difficulty of analyzing political corruption.

In terms of its stated purpose, the report succeeds, in spite of one significant flaw, in providing a set of striking and startling findings. The flaw involves the Commission's failure to define what it means when it talks about police corruption. Nowhere in its report is there a single standard or definition which the Commission employs to judge what is or is not corrupt behavior on the part of the police. As a result, when it states as its conclusion that corruption is so widespread as to be ". . . indulged in to some degree by a sizeable majority of those on the force and protected by a code of silence on the part of those who remained honest" (p. 61), the reader is given no idea of the basis upon which this judgment is reached. Instead of providing a definition or standard which would serve that function, the Commission merely describes a great variety of activities, ranging from burglary to accepting free lunches, which it believes inappropriate forms of police behavior. Readers looking for a precise delineation of the parameters of police corruption will be disappointed.

In spite of the problem of this imprecision in its use of the term corruption, the Commission is able to provide a fascinating description of the situations in which police become involved in clearly questionable practices. These situations include providing protection for illegal gambling establishments, selling narcotics, overlooking building code violations on construction sites, and tolerating illegal parking in the commercial district. According to the Commission, most of these situations, in which the opportunities for corruption are realized, result from an overextension of legal regulation. This overextension in turn is produced by laws against victimless crime and ordinances which create unnecessarily complicated regulatory procedures. These types of laws invite corruption because they require no complaining party; they enable police to exercise unchecked and unsupervised discretionary power. This power, which can be used to facilitate as well as disturb the activities of both legal and illegal enterprises, is itself a valuable commodity, which the Commission believes, gives the police something important to exchange. Because they have this power, many policemen are able to arrange continuing, cooperative relationships with gamblers, prostitutes, and others for whom police forbearance is an important asset; because laws against victimless crimes and complicated regulatory ordinances are on the books, policemen can act as entrepreneurs, offering to "look the other way" and to underenforce laws which many people believe should not be enforced anyway.

Believing as it does that bad laws make bad police, the Commission sets out as a necessary first step in the fight against corruption the repeal of laws against gambling, prostitution, certain forms of drug use and many outdated regulatory ordinances. While there is considerable merit in this recommendation, it is supported by the wrong kind of reasoning. The Commission sets out what is, in effect, a plan to tailor the making of laws to suit the practices and preferences of those who are supposed to enforce them. Such a plan would seem to put the cart before the horse. Furthermore, if the opportunity for corruption inheres in the exercise of police discretion, as the Commission believes it does, then merely changing a few laws will not get at the heart of the problem. If social science research on the police has taught us anything, it is that the exercise of discretion is part of almost every routine police activity. The Knapp Commission, by equating the exercise of discretion with the enforcement of laws against victimless crime, ignores this lesson.

The Knapp Commission, in addition to studying the effect of certain kinds of laws on police corruption, examined the way in which the organization of the New York City police contributed to the problem. It describes in considerable detail the way the N.Y.P.D. is structured and pinpoints three main problem areas in department organization. The first involves the fragmentation of authority and the difficulty of command supervision. The strength of precinct organization in the department makes it very difficult, according to the Commission, for police officials with a "cosmopolitan" perspective to oversee police activity at the grassroots. As a result, corruption often characterizes entire precinct organizations from top to bottom. Second, the absence of an effective intradepartmental system for disciplining officers suspected of corruption reduces greatly the risks involved in engaging in corrupt activities. Third, and most important, the Commission suggests that the special outlook on the world which characterizes the department encourages the development of what might be called a "culture of corruption." This culture is one offshoot of the more general police culture which emphasizes group solidarity and interpersonal loyalty against a hostile and suspicious outside world. Police attitudes, according to the Commission, condone corruption as something everyone does, as a way to compensate for the lack of appreciation which is the policeman's lot.

While the Knapp Commission makes recommendations that seem to speak quite well to the first and second of the organizational problems, the Commission has little of value to say about the third. The Commission's study of corruption is disappointing in at least two other respects. First, the Commission makes no effort to identify and examine linkages between police corruption and corruption in other government agencies or in politics generally. It treats as an isolated phenomenon what may, in fact, be quite general in scope. Second, the Commission presents no evidence as to the extent and nature of police corruption in other cities. As a result, while true to the Commission's limited mandate, its report can serve as no more than a single case study of what may be a widespread problem. In this regard as in much else, the Commission's report raises as many questions as it answers and seems not to have raised many of the hard questions which needed to be asked.

AUSTIN SARAT

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**Professors, Unions, and American Higher Education.** By Everett Carll Ladd., Jr. and Seymour Martin Lipset. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973. Pp. 124. $1.75, paper.)

Collective bargaining among faculty in higher education, though proceeding less extensively and rapidly than among public school teachers, is developing. It is an important subject but one that thus far has occasioned more heated than illuminating analysis. All the more welcome, therefore, is the present volume, slim but meaty, which usefully treats some central concerns. Effectively drawing upon a massive Carnegie Commission survey of faculty in 1969, a close inspection of available campus experience, and selective coverage of the relevant literature, Ladd and Lipset provide instructive description and interpretation.

Among the greatly varied units of higher education, collective bargaining to date has been disproportionately attractive to two-year community colleges. This suggests an explanatory variable emphasized by Ladd and Lipset, namely, that the least profession-like faculty sectors are most receptive to faculty unionism. Two-year colleges provide almost an exaggerated instance of the rule; their status is closer in many ways to the K-12 public school system than to higher education, leading their faculty to support of unionization to enhance their professional standing.

A changing environment helps explain the growth of faculty unionism in four-year colleges and universities, allowing for the fact that a sizable segment of the professoriate remains opposed to unionization on traditional grounds. Readers need no reminder that the boom period for higher education has ended and that economic retrenchment is the order of the day. Faculty concern over protection of its prerogatives and standing has been further heightened by the student power thrust and by the enveloping mood of egalitarianism and antielitism. In the public higher education sector, the site of nearly all the faculty collective bargaining that has occurred, two additional explanations may be offered. One is the increasing scale of higher education units, including gigantic multicampuses, which has promoted bureaucratization and impersonalization, has boosted central authority at the expense of campus autonomy, and has reduced collegiality between faculty and administration. The other is the enactment by nearly half the states of legislation authorizing collective bargaining by public employees and/or those in higher education, which has spurred organizing elections on some public campuses. Although faculty at private institutions have had the right to organize under NLRB jurisdiction since 1970, collective bargaining has made little inroads in that sector.

The broad lines of faculty support and opposition on faculty unionism follow class interest: "the lower the tier of academe, in terms of security, income, prestige, and involvement in the graduate scholarly-research culture, the stronger the vote for unionization, as represented by a regular union body [like the AFT or the NEA]; the higher the level, the greater the likelihood of votes for 'no representation,' or for the least 'union-like' faculty organization on the ballot" (p. 49). In addition, say Ladd and Lipset, the political attitudes of faculty members comprise an independently influential explanatory factor: liberal-to-left professors are more open to collective bargaining than are faculty of a politically conservative outlook. The two variables interact in interesting ways. Faculty at elite universities, for instance, tend to be more left-wing politically than faculty elsewhere (which moves them to favor unionism in principle), but at the same time they are most deeply attached to academic norms of achievement and merit (which move them to oppose unionism and its leveling ideology). Thus far, Ladd and Lipset show, their conflict has been resolved mostly in favor of class interest, i.e., no unionism. In contrast, generally the younger and untenured faculty find their political beliefs and their class interest reinforcing, which makes them as a group more receptive to the advent of faculty unionization. Interestingly, the class origins of faculty fail to explain intrafaculty variation on unionism; the authors' analysis indicates that professional socialization seems to be far more controlling.

Intrafaculty differences on unionism underscore the practical importance of unit composition to the outcome of organizing elections. The election results may turn, the authors remind us, on whether, in a multi-campus university system the unit is defined as the regular faculty in each campus separately or in the system as a whole. The same comment applies to situations in which the university faculty and the faculty of the other public higher education institutions are assigned to separate units or combined into one. Similarly, different election outcomes are likely if the regular faculty members are merged with other teaching/research/professional staff in a single unit or are permitted to comprise their own unit. The determination of the unit can be set directly in the state legislation authorizing collective bargaining or can be assigned to a state board, subject to criteria specified in the legislation. Whichever is the case, faculty organizations would be wise to move early to shape a legislative position on unit definition protective of their particular needs and interests.

What are some expected effects of faculty unionism, especially if an industrial union model is approximated? Only experience can tell, but in considering the speculations of Ladd and Lipset and of other writers on this subject, the following union means or objectives seem to me to be highly plausible probabilities: (1) to eliminate salary differentials (merit raises, etc.) among those in a given job category, with the exception of a recognition of seniority in service; (2) to make reappointment, tenure, and promotion more automatic in terms of time served, with the institution assigned the burden of proving a negative decision; (3) to insist on open personnel files, easier grievance procedures, and a more formalized process for review of faculty; (4) among more marginal faculty groups (lecturers, researchers, etc.), to move to attain the status and privileges of the regular faculty; (5) to increase an adversarial posture between faculty as "employees" and administration as "management," though it should be noted that some redistributions of power would be from the senior tenured faculty to the junior faculty; (6) to pursue higher salaries and other economic matters by direct dealings with the legislature and governor; (7) to develop a type of faculty leadership different from the "institutional oligarchs" in charge of faculty senates, and to hasten the decline of such traditional modes of institutional and faculty governance; finally (8) to reassert and maintain faculty prerogatives on a wide range of institutional and policy matters, and from a vantage point not necessarily consistent with the interests of students or of the public.

Whether faculty collective bargaining is viewed as boon or bane, as degrading or promoting professionalism, its potential for altering higher education and the role and place of faculty cannot be denied. Hence political scientists would do well—in the manner of Ladd and Lipset—to monitor its evolution closely and perhaps even to shape its development by helping to construct a new type of unionism appropriately sensitive to what so many of us refer to as the "special" character of higher education teaching and research.

ALLAN P. SINDLER

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**Community and Regional Planning: Issues in Public Policy.** By Melvin R. Levin. (New York: Praeger Publishers, 1972. Pp. 362. $15.00.)

Levin's book is a collection of fourteen of his essays, written primarily during the mid-1960s and updated by occasional reference to more current events for this second edition. Its main focus, however, is programs, events, and issues of the 1960s. The somewhat dated quality is counterbalanced by the opportunity the volume gives us to see what was on the mind of a professional planner and how he defined and responded to the public policy problems of that era. The range of topics Levin covers is very wide: the role and availability of intellectual talent in state government, PPBS and its antecedents, planners and metropolitan planning, transportation factors as independent variables in institutional planning, aspects of distressed regions and regional commissions, and various problems of state and local governments.

Even the above listing does not indicate Levin's scope. He is a discursive, undisciplined, and unedited writer. For example, in his essay on "Costs, Benefits, and Social Indicators," he connects PPBS to its ancestors in the cost-benefit analyses of water projects in the 1940s and 1950s, then discusses indicators in general, then revenue sharing and the concept of governmental decentralization, slips off into an analysis of New York city problems, and ends with a case study of what is wrong with administration in Massachusetts state government, with details of capital budgeting and data processing shortcomings. Or, in "The States and Urban Programs," Levin comments upon: current events in the Nixon Administration during 1971–1972, Patrick Moynihan's ten-point urban program, the creation of the Domestic Council, the ambiguities of the word "policy," the problems of urban political action, and the role of the private sector and of research in problem solving. He then archly declaims (p. 297): "Now that the federal government has been properly discussed, it is time to start on the states. Clearly, any discussion of state problems runs the risk of generalizing the way sophomores do about women or some people do about Negroes, Chinese, or other seemingly discrete groups or categories." Levin then moves on to discuss the weaknesses of state governments and to deliver some prescriptive maxims for policymakers (policy should run *with* rather than counter to developmental trends, there must be more emphasis on public affairs education in the schools).

This book will be maddening to any political scientist hoping to find hypotheses to test against data or any rigorous use of empirical methods. In essays on distressed regions, there are solid data on employment and population mobility. But throughout the essential mode is that of the "authority" describing any aspect of the passing scene that strikes his fancy and rendering strong *ad hoc* judgments and prescriptions.

The causal diagnoses that result, and their philosophical implications for the planning discipline are very disturbing. Levin's constantly reiterated conclusions amount to one point: the problems of state governments, of the regional commissions, of federalism, stem from an absence of "talent." He says (p. 307) that "this may neither be the first nor the last time in history that noble concepts and imaginative plans were degraded by faulty execution, but there is something rather odd in the spectacle of major programs in a major nation faltering for want of a few thousand trained professionals."

How do we know there has not been enough "talent?" By what absolute or relative standards can these judgments be made? There are no data in this book on the characteristics of personnel in terms of education, experience, or attitudes. There are no indications why "talent" is the explanatory variable in assessing the impact of federal-state, state, or urban policies, or, indeed, what the criteria for evaluating these policies as either successes or failures might be. Although Levin is aware of the impact of bureaucratic rivalries upon policy in the abstract, there is little attention paid to political processes in setting and administering programs.

What we have then is an unintended substantiation of Alan A. Altschuler's thesis in *The City Planning Process* (Ithaca: Cornell University Press, 1965). While planners assert judgments upon all aspects of public life, their perspective has no explicit theoretical underpinnings. What comes forth are aggressive assertions without substantiation, the net effect of which is argumentation in defense of the planner's own biases. One can be intrigued with and even sympathetic toward the values of Professor Levin. But it is hard to see how the essays of *Community and Regional Planning* advance our understanding of public policy at any level or of the rationale for planning.

JAY S. GOODMAN

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**American Labor History and Comparative Labor Movements: A Selected Bibliography.** Edited by James C. McBrearty. (Tucson: University of Arizona Press, 1973. Pp. 262. $7.50.)

This work organizes 3,187 sources under two major headings, American and Comparative. American listings are grouped by chronology, although the "Recent" section is topical as is a lengthy "Special Topics" section. These topical sections include subjects such as "public employees," "foreign policy," "immigrant and minority groups," "cities and states," "labor and the law," and "labor and politics." The Comparative listings are grouped by country, except a brief listing of general surveys. This whole system of categorization is repeated twice: once for books and once for articles. I found this feature a drawback, since it necessitates looking up a given topic twice. An index of authors is also included.

McBrearty excludes foreign-language publications, and, in general, the book is weighted toward American labor studies. Emphasis is on academic sources, although business, professional, and radical sources are given attention. Primary sources are not included in the survey. McBrearty, an economist and professor of labor law, gives due weight to these subjects in particular.

Apart from general categorization, there is little to guide the unfamiliar user. Citations are not annotated, nor are there analytic introductions to sections. For articles, there is considerable overlap with *Labor History's* annual review, though both references may be consulted with profit. *Labor History,* it may be noted, reviews *Dissertation Abstracts,* which appears to be omitted from McBrearty's volume. *Labor History* also seems to me to be a better source for articles on relations of labor to the left. Overall, this volume provides a helpful but intentionally not comprehensive starting point for researchers laboring in these fields. Political scientists, notorious for their benign neglect of labor studies, may benefit particularly from the basic sources conveniently set forth in this reference work.

G. DAVID GARSON

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**Independence on Trial: Foreign Affairs and the Making of the Constitution.** By Frederick W. Marks, III. (Baton Rouge: Louisiana State University Press, 1973. Pp. 256. $10.00.)

The rejection of the Articles of Confederation and the subsequent adoption of the Constitution in 1787 have been viewed historically as dissatisfaction with socioeconomic conditions in the early stages of this nation. This position has been made popular through Charles Beard's classical study, *An Economic Interpretation of the Constitution of the United States* (New York: Macmillan, 1913). Some political historians, however, are beginning to consider the impact of foreign affairs as perhaps the most important variable in the Constitution's drafting. Beard's assumptions and methodologies also have been criticized by Robert E. Brown in *Charles Beard and the Constitution: A Critical Analysis of "An Economic Interpretation of the Constitution"* (Princeton: Princeton University Press, 1956) and Richard B. Morris, "The Confederation Period and the American Historian" *William and Mary Quarterly,* 13 (April, 1956), 139–56. Moreover, other authors are starting to reinterpret the significance of Shay's Rebellion in both its impact and possible reflection of foreign intervention by the British in 1786.

Frederick Marks III in *Independence on Trial* does not seek to minimize the role that socioeconomic conditions have played. In fact, he states that the traditional theory still endures and is the subject of a most recent book by Gordon S. Wood, *Creation of the American Republic, 1776–1787* (New York: Norton, 1972). But the author contends that problems of foreign affairs—British trade restriction, national security factors, patriotism—should be viewed in conjunction with the theories of Beard and Wood.

The Articles of Confederation sufficiently decentralized the power of government so that Congress could not provide for the national defense of the country. It lacked, according to Professor Marks, the ". . . two prerequisites for a satisfactory national defense under the Articles. The first was exclusive control of foreign relations; and the second, an independent source of revenue" (p. 47). As the author indicates, although Congress had the treaty power, state governments acted independently in violating British-American agreements and federal Indian policies. Furthermore, since Congress could not tax and raise revenue, the entire range of national security problems—including troop size, military negotiations, and the national debt—suffered. It was the weakness of "national insecurity" which proved to be a major factor in the demand for constitutional reform.

The British, according to Marks, were aware of Congress's inability to act on military and trade problems. Lord Sheffield contended in *Observations on the Commerce of the American States with Europe and the West Indies* (London, 1783) that:

> No treaty can be made with the American states that can be binding on all of them . . . When treaties are necessary, they must be made with the states separately. Each state has reserved every power relative to imports, exports, prohibitions, duties, etc. to itself (p. 55).

The trade advantages were apparent and they partially closed the West Indies to American ships on July 2, 1783. Marks contends that when the individual states later sought to counter with their own protective tariffs, they found their efforts conflicting. Each state sought trade advantages vis-à-vis the other and the British were able to benefit.

It is the author's view that the failure to set a common trade policy led to the Annapolis Convention in September, 1786. Because of the absence of some delegations, major decisions were postponed. Those that did attend, proposed a constitutional convention to be held in Philadelphia. Hamilton argued, in the final report at Annapolis, that the new convention should not be restricted to trade factors, but should also consider the "other parts of the federal system" (p. 93). Foreign trade problems were viewed as being interrelated with the general need to broaden national power. Professor Marks states that a consensus developed whereby the federal government would be given increased authority in "taxation, military establishment, regulation of foreign commerce, and treaty enforcement" (p. 143). Although there were differences in particular plans, the need for reform was recognized in Philadelphia. The final document was viewed as a "tonic for the national spirit," as it would alleviate the major problems of the Articles.

Marks asserts that the weakness of the American defense and the commercial disadvantages that were apparent under the Articles were two of the primary positions used by the Federalists on behalf of ratification. Madison, Hamilton, and Jay argued in the *Federalist Papers* on the need for a more unified government that would stress common interests and be capable of negotiating with foreign powers. The anti-Federalists, according to

Professor Marks, did not address themselves to questions of foreign affairs nor issues of national security. The author cites works by Richard Henry Lee, George Mason and Elbridge Gerry as examples of anti-Federalist writers who were concerned more with domestic considerations and the possibilities of governmental centralization. Social and economic issues were important, he feels, but the increased national power was ultimately justified because of the necessities of foreign affairs and national security. The benefits of ratification were an improvement in the nation's military, economic, and trade positions.

*Independence on Trial* is a well-written and highly informative study of the impact of foreign affairs on the writing and ratification of the Constitution. The author provides an interesting interpretation, good documentation, and a solid bibliography. This rewarding work is a fine contribution to political science.

Carl L. Figliola

*C. W. Post College*

**Managed Integration: Dilemmas of Doing Good in the City.** By Harvey Luskin Molotch. (Berkeley: University of California Press, 1973. Pp. 250. $8.95.)

Professor Molotch's *Managed Integration* is a sobering and instructive book. It is sobering because it documents the futile efforts of a citizens' commission in Chicago's South Shore to "manage" in-migration to their racially changing community and to maintain its solidly middle-class character by retaining white households. The book is instructive in several respects: (1) some of the tactics used by the South Shore Commission are transferable to other areas; (2) some of the commission's strategic errors pointed out by Molotch could be avoided by community groups elsewhere; (3) the commission has not attained its original goals in South Shore, but several of its achievements have substantially improved the neighborhood environment for newly arrived residents; and (4) the South Shore example adds to the evidence of the limited capability of local neighborhoods to change—or, often, even to influence—the direction of powerful metropolitan-wide trends.

South Shore, as the name implies, fronts on Lake Michigan in the southern part of the City of Chicago. It is below Hyde Park and, like that community and other lakefront areas to the north of the central business district, was an affluent neighborhood of mansions, high-rise buildings with large apartments, and substantial middle-class homes and apartments. Unlike the lakefront areas north of downtown, South Shore was bounded on the west by Chicago's expanding black ghetto. Unlike Hyde Park just to the north, which contains the University of Chicago, South Shore had no established, expanding institution to generate white residents, employees, and visitors.

In 1960, South Shore's population was 99 per cent white. It is now estimated to be 80 per cent black. As Molotch effectively shows, this rapid change was *not* the result of white flight. Instead, whites stopped moving into the area, and most of the market vacancies were filled by blacks. Since nearly 20 per cent of America's households move each year, the occupancy of a community can change quickly without any acceleration of the normal turnover rate. Renters tend to move more often than homeowners, and half the units in South Shore are in multifamily buildings; so occupancy has always changed in a high proportion of the community's housing units each year. The rate did not increase during the period of racial change.

To evaluate the comparative stability of the real estate market in South Shore in 1966, Molotch performed a careful analysis of single-family home transfers in Rogers Park and South Shore. The two communities are directly comparable, except that Rogers Park is on the north lakefront—at about the same distance from the central business district. Molotch found that, in comparison with Rogers Park, South Shore had no more home transfers, no more "for sale" signs, and no price decreases during the period of rapid racial transition. His conclusion after this analysis was, "South Shore is stable; there are no signs of flight by homeowners from the racially changing neighborhood" (p. 161).

Molotch's evidence was treated with skepticism by community residents, who perceived outmigration as being a major problem in South Shore. Presumably, this mistaken perception arose because the in-migrants were visibly different, and residents were made more conscious of migration patterns that were less obvious when movers-in were of the same general socioeconomic and racial mix as their predecessors.

Except for the western edge of the community, most of the black households entering South Shore were middle class. Their overall net worth was probably below the white households they replaced, and more of the couples were joint wage earners. However, the community concerns and desired neighborhood amenities of the blacks moving into South Shore were similar to those of the white residents. It is six or seven years since Molotch analyzed South Shore, and the population has become predominantly black. Now the community is fighting a new battle—to retain its middle-class character and to maintain the property values that prevailed at the time of racial transition. Although home prices and rentals did not initially decline (according to Molotch's evidence, they may even have increased), there have been secondary effects of racial transition. The business and financial communities have not written off South Shore, but neither have they continued the level of reinvestment that is usual for a middle-class white community. Thus, today's residents are anxiously working to keep the area a viable and healthy middle-class black area.

In the last 20 years, the focus of the South Shore Commission has changed substantially. At its inception and through its energetic period in the mid-sixties, the Commission, according to Molotch, "attempted to create neighborhood conditions it thought important to attract whites and to make these neighborhood assets generally known to prospective white residents. Secondly, an effort was made to directly facilitate white move-ins by recruiting white, middle-class residents through a tenant referral service and other means" (p. 83). The direct accomplishments of the Commission in this period include: establishment of good relations with political leaders in the City of Chicago, construction of a new South Shore High School, generation of community concern in local school administrators, creation of a central screening service for apartment applicants, execution of a stiff city building-code enforcement program, and publication of a monthly newsletter that was sent free to 25,000 families and businessmen in the community. These accomplishments did not stem racial change, which was a goal of the Commission. However, they certainly contributed to preservation of physical and social amenities in South Shore.

The South Shore Commission is still an active force in the community, but its goal is not so much to achieve integration as to prevent economic deterioration. The Commission managed to orchestrate purchase of the large South Shore National Bank to avoid the loss of the only neighborhood bank. It is now working on purchase of the failing South Shore Country Club by the Chicago Park District to expand the recreational and educational opportunities in the community. The Commission is now more representative than it was when Molotch was studying it, but its primary concern is still neighborhood preservation. The emphasis is no longer preservation of a biracial community (Molotch demonstrates that South Shore was never really integrated), but preservation of a middle-class community.

Integration was probably a hopeless goal in South Shore, given the community's location on the edge of the black ghetto and the many housing options available to middle-class whites in the Chicago metropolitan area. Despite the apparent failure of the Commission's original intent, however, Molotch's analysis of the organization is valuable in terms of both achievements that are possible and limitations that are inherent in a citizens' action group.

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Anthony Downs
*Real Estate Research Corporation*

**Corporate Power and Social Change: The Politics of the Life Insurance Industry.** By Karen Orren. (Baltimore: The Johns Hopkins University Press, 1974. Pp. 204. $10.00.)

Despite the efforts of governmental institutions at all levels, the problem of urban decay and minority advancement have stubbornly resisted solution. According to Professor Orren, the impact of government has been marginal because the causes of the problems are to be found in institutions and practices over which it has exercised little control. Since many of these mechanisms are the province of corporate business, Professor Orren's book sets out to investigate whether voluntary corporate action can constitute an effective instrument for social change.

While there is a wide range of corporate activities that might be subject to voluntary modification to bring about reform, this book focuses on the mechanism of credit. The life-insurance industry has been chosen as the focus of the analysis both because it controls vast amounts of investable funds and because its $2 billion Urban Investment Program has been one of the most prominent examples of voluntary corporate social action.

Since the nature and extent of corporate voluntarism are critically dependent on the relationships between business and government, the book begins with an analysis of those relationships in the case of the life-insurance industry. Utilizing the state of Illinois as an example, the author documents the lobbying activities of the companies and indicates how they have attempted to protect themselves from incursions into their corporate autonomy. That discussion is followed by a chapter on "the life insurance enterprise," which analyzes the characteristics of the life insurance business. The two subsequent chapters investigate life insurance company mortgage investment patterns in the Chicago area and consider the nature and results of the Urban Investment Program.

These latter two chapters constitute the primary contribution of the book. In the chapter on mortgage investments, 129 census tracts were examined over the thirty-four year period 1935 to 1968 to determine the incidence of life-insurance company mortgages in various parts of the city and its suburbs under changing socioeconomic conditions. This analysis provides significant evidence of institutional racism. Even when factors such as the income of the mortgagor and the age and value of the properties are taken into account, the data indicate that life insurers have consistently favored whites over blacks in their mortgage lending activities.

The discussion of the Urban Investment Program credits the companies for their efforts and acknowledges their sincerity. It also demonstrates, however, that the program must ultimately be regarded as a failure. Plagued by the lack of clearly defined goals and the absence of an authoritative coordinating body, the program led to disillusionment among the companies and resentment among blacks. One of the problems was the decision by the companies not to lend funds under the program at less than market rates. As the author points out, "while neighborhoods may be built, maintained, and even allowed to decay at market rates, it takes something more—or, more precisely, something less in the way of private profits—to put them back together again" (p. 160). Thus, instead of providing a real impetus for the solution of the urban crisis, the companies "channeled funds straight to the most profitable or peripheral segments of the ghetto community, most of which were already served adequately in financial markets" (p. 183). These considerations lead the author to suggest that the purely voluntary approach is not the final answer to the problems of the cities and that a greater degree of governmental coercion will probably be necessary in order to harness the resources of the private sector for the public good.

Unfortunately, the author's accomplishments in the chapters on mortgage investments and the Urban Investment Program are not matched in the chapters on life insurance politics and the nature of the life insurance business. While the chapters on politics provide some revealing glimpses into life-insurance industry lobbying activities, they contain several highly questionable theses. For instance, one of the primary contentions is that "a particular mode of legislation—the mandatory investment law—has largely determined the companies' preference for state or federal regulations" (pp. 34–35). To demonstrate this the author indulges in proof by omission, i.e., she shows that there has been concern among companies over mandatory investment laws but virtually ignores other aspects of regulation that may have influenced company opinion. In fact, most observers would argue that mandatory investment laws have constituted little more than a peripheral issue in the broad scheme of regulatory conflict.

Another dubious conclusion is that in insurance regulation "safety is not a policy but rather a pseudopolicy . . . devoid of clear procedural or substantive comment" (p. 67). Here again the author has drawn a sweeping generalization based on only partial evidence. While one can raise legitimate questions with regard to the success or indeed the fundamental design of existing mechanisms for solvency regulation, there can be little question that safety has long been the overriding concern of insurance regulatory law and practice.

Unlike the chapters on life insurance politics, which are partially redeemed by a number of insightful comments, the chapter on the life insurance enterprise has little to recommend it. That discussion is characterized not only by half-truths and outright errors but by misinterpretations of those facts which have been recorded correctly. The problem here as in the regulatory chapters seems to be that the author has overlooked several of the most significant prior scholarly treatments of the subject matter.

As serious as the defects of the regulatory and business enterprise chapters are, however, they do not detract from the value of the chapters on the racial implications of life-insurance company mortgage investments and on the Urban Investment Program. The book is clearly worth reading for those two chapters alone. On the other hand, for the reasons stated above, the chapters on life insurance politics should be read selectively; and the reader might be well-advised to avoid altogether the chapter on the life insurance enterprise.

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**The Future of the United States Government.** Edited by Harvey S. Perloff. (New York: George Braziller, 1971. Pp. xxiii, 338. $7.95.)

This book is a product of a working group of the Commission on the year 2000 of the

American Academy of Arts and Sciences. Nineteen papers are presented in the volume as well as six transcripts from the working group's discussions. The essays contained in this work are essentially speculative, predictive, and prescriptive in character and no attempt is made to separate fact and value in any definitive way. The volume is given balance by including papers contributed by individuals with business and governmental experience as well as by scholars from several disciplines.

Professor Perloff, editor and contributor, in an excellent introductory essay provides the framework for the working group's essays and discussions. According to Perloff, key problem areas of the future include pressures on the individual, the danger of deterioration of the internal cohesion of society, deterioration of the environment and mass politics, and the domestic strains of world responsibility. Fundamental to any effort to solve these and related problems, in his view, is a basic alteration in our public philosophy. Society must first broaden its conception of civil rights to include for providing each individual the opportunity to develop his potential to its fullest. A new public philosophy must also provide for more freedom for individuals and groups. This might take the form of communities within communities maintaining their own "life-style" groups through self-discipline. Coupled with this new freedom is the need for a better-defined and stronger sense of social obligation among individuals in society. Our foreign policy, he believes, must also be restructured toward a more pervasive concern for human rights and dignity. Finally, the United States should become more aggressive in promoting world economic integration since a well integrated economic community will be less prone to political conflict.

Taking this framework as their point of departure, most contributors focus on how the American political system can be reorganized and restructured to maximize such values as full opportunity and greater personal freedom. Among the many prescriptions for change suggested in the volume, several emerge as overridingly important. American political institutions, it is argued, must become more sensitive to individual needs and more opportunity for innovation must be created at all levels of government. Addressing himself to this issue in one of the volume's more interesting essays Donald A. Schon argues for the development of "pools of competence" based on skills of competence (e.g. statistics and computer programming) and process (e.g. planning and program management) to deal with problems which do not fall within the boundaries of established agency structures. Several of the essays project that by the year 2000 distinctions between public and private organizations will become increasingly blurred and private organizations will preform more activities which have traditionally been considered public in nature. Government, according to an essay by Robert A. Nathan, will by the year 2000 be primarily involved in setting broad standards and policies aimed at achieving self-regulation in the private sector while the private sector will be the principal agent for economic and welfare activities. Many of the essays also stress the point that in future decades political participation will need to be broadened to include individuals and groups such as the poor, the aged, women, and the socially disadvantaged who are now only marginally politically active. Also, many contributors believe that state governments now in the process of atrophying, should become more rigorous, while local governments should decrease in number, consolidate many of their functions and become somewhat more autonomous.

Although there are many positive aspects to the individual papers, the volume taken as a whole has certain problems. Perhaps most significant is the fact that the book's scope is too broad. It is difficult to conceive of a volume dealing with the American presidency in the year 2000, let alone the entire structure and process of the American political system. As a result, some of the subject areas are treated in a thin and superficial fashion. A second problem involves the tacit assumption by the working group that with certain minor fluctuations the American economy will remain healthy, and real economic growth will continue unabated. Clearly in an era of double-digit inflation and economic slow down this assumption with all its implications can be severely questioned.

Viewed as a ground breaking effort, however, the working group's essays are food for thought. Political scientists in particular and social scientists in general who are interested in the area of political and social change will find them worth lingering over.

GRANVILLE J. FOSTER

*Columbus College*

**Old-Age Politics in California: From Richardson to Reagan.** By Jackson K. Putnam. (Stanford, Calif.: Stanford University Press, 1970. Pp. 221. $7.50.)

This brief but well-documented historical study recounts the growth of welfare legislation for the aged in California as well the often emotional politics, both conventional and unconventional, accompanying that growth. Only the Depression period, from Upton Sinclair's EPIC (End Poverty in California) campaign in 1933 to the demise of the Ham and Eggs movement in 1940, is covered in detail. The fifty years preceding and the thirty years following that period are treated summarily in two chapters. The most original contribution of the monograph is the description of the Ham and Eggs movement, which has never before been studied systematically.

Although claiming the historian's right to be conceptually eclectic, Professor Putnam attempts to place California's old-age politics in the context of some social theory or theories. How does one explain the at times bizarre old-age movements with their mixture of religious revivalism and crass commercial promotion tactics? How could some of these movements come so close to taking over the welfare machinery of the state? Why did they leave such telling marks on California politics? More generally, what accounts for the saliency of the old-age problems in the political life of California? Putnam argues that the purported high concentration of old people in California (note that, since 1960, California's proportion of persons over sixty-five years of age is below the national average) constitutes at best a very partial explanation. As for the notion that the presumably blocked activity needs of the aged come to be expressed in political participation—this explanation seems, in the absence of direct evidence, at least highly doubtful. Putnam applies similar strictures to explanations based on personality variables or feelings of isolation and estrangement from society. Finally it is unclear, as the author points out, why the disengagement hypothesis—the need of the aged to reduce their contacts with the social world—should ever have been considered in connection with increased political participation.

Elsewhere, Putnam shows that conventional politics—constituency and interest-group pressures in the legislative process—have yielded more benefits to California's aged than have the various special interest movements, from the Townsend plan to the California League of Senior Citizens of George McLain and Myrtle Williams. Thus, certain legislators, such as San Francisco assemblyman William B. Hornblower in the 'thirties, have consistently drafted, and marshaled the necessary political support for, bills liberalizing old-age grants and programs. Putnam states repeatedly that during the period he considers, a politician's stand on old-age benefits could make or break him, since a substantial number of electoral constituencies favored higher grants and more liberal eligibility provisions. He offers no explanation for this peculiar state of affairs. The answer may well be that the process of age segregation was particularly rapid and severe in California, and that this process created numerous constituencies with high concentrations either of impoverished old people or of younger couples concerned about the economic situation of their distant parents. This hypothesis, if true, might also help to explain why old-age issues should be so much more prominent on the political landscape of California than on that of other states—a problem with Putnam entirely fails to confront.

If the aged have had some political success in California—so Putnam concludes—it is because they were able to form an effective interest group. But whether the activism of old people has actually contributed much to their cause remains open to question. California's old-age movements, at any rate, have been typically neither initiated nor run by old people. They have typically taken the form of proprietary organizations operated by their founders ("younger" people for the most part) with the help of organizers or speakers working for a share of the "take" on collection plates or in membership fees. In addition, plans and solutions advocated by these organizations, whether ridiculous or sublime, have tended to be utterly impractical. The opprobrious term "pension promoter," so common in the political vocabulary of California, was therefore not entirely unjustified. Putnam correctly points out that pension organizations have become more and more responsible; indeed, the lobbying efforts and pensioner service activities of the League of Senior Citizens have been competently organized and carried out. Still, to the extent that pension organizations have given up their millenarian appeals, they have lost the mass followings they once enjoyed.

One can only conclude that it is not the activity of pensioners in their own interest that accounts for their modest success. Rather, the problems of the aged were placed high up on the political agenda and remained there because of the clear definition of the political constituencies that are sensitive to them. The point is worth making because it may apply, in varying degrees, to other diffuse interests, such as those of battered children or consumers.

FRANK A. PINNER
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**State Legislative Innovation: Case Studies of Washington, Ohio, Florida, Illinois, Wisconsin, and California.** Edited by James A. Robinson. (New York: Praeger Publishers, 1973. Pp. xvii, 281. $17.50.)

This anthology brings together under a single cover diverse reports having to do, not with innovation, but with fairly mundane legislative reforms and other changes. All of the changes involved are well-worn ones, and the essays in this book are analyses of legislative changes in selected states. Douglas S. Gatlin writes about changes in the legislative party system in Florida, focusing on the 1967 session. Alan J. Wyner deals with changes in the 1960s in the California legislature, and especially with reforms associated with the leadership of Jesse Unruh. Samuel K. Gove and James J. Best report analyses of the effects on public policy of legislative reorganization and reapportionment in Illinois and Washington. Alan Rosenthal analyzes the effects of increased professional staffing upon state legislative performance in Wisconsin. Finally, Thomas A. Flinn considers the extent to which the Ohio legislature has become a "professional" legislature. Quite apart from the fact that they are not integrated in any way, and no conceptual or theoretical synthesis is attempted, these reports do provide valuable and useful material on individual state legislatures.

The following seem to be the main findings which can be drawn from these reports of research in individual states:

(1) Change from a single-party legislature to a two-party legislature, as in Florida, requires more than simply the election of a critical mass of opposition party members. Two cohesive legislative parties can develop fully only if (a) party leaders adopt and assert clear and distinctive policy positions, and take on conflictful party role behaviors, (b) legislators strongly identify with their party, (c) rewards and sanctions are distributed in accord with the party's policy positions, and (d) party caucuses and leaderships reinforce a consistent party program.

(2) Changing a legislature in the manner in which the California legislature was "reformed" in the 1960s—providing a substantial staff, having annual sessions, streamlining committees, strengthening party leadership, raising legislators' salaries, providing adequate facilities—may result in (a) only modest relative increases in costs for the legislative institution, (b) some increase and better distribution of the workload of bills, and reduction of the end-of-session "logjam," (c) a legislature more independent of the executive and private interest groups, (d) lower turnover of members, (e) the effective end of the part-time, "citizen" legislator, and (f) no clear, substantive policy effects attributable to the reforms.

(3) A legislature resistant to internal reorganization in the direction of greater "professionalization," as in Illinois, is one in which marginal procedural changes are especially unlikely to have discernible policy impacts.

(4) When a state has been egregiously malapportioned, as Washington was prior to 1966, reapportionment may have a policy impact in improving the chances for enacting urban-oriented legislation. But, in enlarging the representation mainly of suburban areas in northern urban states, reapportionment may have as one of its main effects a considerable, and perhaps appropriate, enhancement of Republican membership in the legislature; and the landslide 1964 presidential election obscures the pure effects of reapportionment to a substantial but unknown extent.

(5) The expected unintended or latent consequences of substantially increasing the party and committee staffs of a state legislature like that of Wisconsin—increased workload and increased partisan conflict—can, indeed, be shown to develop, so that increased professional staffing may result mainly in making the legislature more partisan, particularly by enhancing the capability of the minority party to oppose the majority.

(6) The "professionalization" of a legislature like the one in Ohio in terms of the standard reforms of the formal structure may not have associated with it some of the informal changes which might be anticipated by virtue of the construct "professionalized legislature." For instance, the Ohio legislature clearly is more professionalized than it used to be, yet turnover of membership remains high, and the level of aggregate positive attitudes toward specialization among legislators has not increased. There is some evidence that the occupational status of legislators, legislative role orientations, distance between legislators and interest groups, legislators' willingness to support governmental activity, and legislators' feelings of competence covary in the expected directions with changes in legislative structure toward a more professional legislature.

Some of these findings are more interesting than others, but what they all do is to reinforce the urgency of conceptual work on processes of institutionalization and other organizational changes. In perhaps rudimentary ways, the research reported in this collection focuses upon institutions changing over time, a much-needed perspective for political analysis. But the *modus vivendi* for each of these projects is largely conventional, and the analytical results are sometimes tedious and always conjectural. Without some theory about changes in institutions over time, a deficiency of the discipline more than an inadequacy of these investigators, it is difficult to give larger meaning to the research presented here. If, as the editor of the anthology suggests, an important yield of this research is the rediscovery that "the more things change, the more they stay the same," we shall not have gained much. If, however, these studies, ingenious and insightful in many respects, contribute to a renewal of interest in constructing empirical theory of institutions and organizational change, then they will have made something of a mark.

SAMUEL C. PATTERSON
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**Position Classification: A Behavioral Analysis for the Public Service.** By Jay M. Shafritz. (New York: Praeger Publishers, 1973. Pp. 133. $12.50.)

Position classification in government is beloved by few, denounced by many, but necessary to most. In this brief new volume, Professor Shafritz adds his whip to the scourging party that has gone on ever since systematic job evaluation was first tried in government. His message is mainly that classification is one feature of bureaucratic management that prevents optimum relationships among employees and optimum development of the individual. This is a good point, but Shafritz pounds it into the ground with repetition and (for my taste) overdocumentation. This scholarly overkill is not all bad. His footnotes and bibliography are well-selected guides to the literature of organization theory as well as that of public personnel administration.

After briefly stating his hypotheses and outlining the history of classification, the author moves to the "inherent dysfunctions"—how flexible people are poorly served by the job-based inflexibilities of the system. Management's ability to use pay as a reward for good work performance is inhibited by the difficulty of reclassifying jobs. Coming to organization theory, he shows how position classification supports hierarchical organization which suppresses conflict, discourages vertical communication, and generates anomie.

The theoretical discussion is then interrupted by a chapter on "Philadelphia as a Case Study." The pressures and manipulations required to get a raise for somebody in the employ of that city are outlined in some detail to show that the classification audit system is bent to get raises for those favored in the organization's internal political system. This chapter demonstrates how the classification process in that government fails three tests of a formal structure suggested by the writings of Herbert Simon: (1) encouraging constructive formation of informal organizational relationships; (2) setting limits on the development of informal relations; and (3) preventing the development of deleterious organizational politics. This chapter is not needed by readers who have been personally exposed to job evaluation processes within governmental bureaucracies, but it is a useful dip into realism for those without such experience.

Returning to organization theory in the sixth chapter, Shafritz shows how classification doctrine and process impede the type of people-centered administration urged by Frederick Herzberg and Douglas McGregor.

The work concludes with a chapter called "Position Classification in the Postbureaucratic Era." How's that? If we ever have such an era, it's not likely to feature classification. Yet Shafritz's solution is to urge that classification be put in the hands of program managers as a tool of administration and that the personnel or civil service staff serve as consultants in its use.

This book is useful as far as it goes—mainly in showing how classification can get in the way of effective management. The author fails, however, to carry far enough his discussion of modern organization theory. This would bring him inevitably, I think, to the conclusion that people-centered, task-oriented, highly flexible management will require a greatly "weakened" job evaluation system. Such a system would feature fewer and broader classes, fewer barriers to movement between classes, and lessened dependence of pay upon job ratings.

My final criticism, I admit, puts me in the classification (that word again!) of reviewers who say "this is not the book he should have written"—but only to the following extent: Shafritz (except for a few words early in the book) does not face the implications of the developing power of public employee unionism on pay practices and levels, including classification, as a partial determinant of pay. Unions have seen to it that blind alley jobs are opened up, some lower jobs upgraded, and new jobs created—all through bargaining and other pressures. Yet Shafritz's discussion implies that management can unilaterally decide what the nature of the classification process will be. Moreover the treatment of organization theory ignores the management-union adversary relationship and the growing influence of unions on assignments, promotions, and employee discipline. Professor Shafritz needs to prepare a sequel to deal with these matters.

DAVID T. STANLEY
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**Seeds of Extinction: Jeffersonian Philanthropy and the American Indian.** By Bernard W. Sheehan. (Chapel Hill: The University of North Carolina Press, 1973. Pp. 301. $11.95.)

Most writing on the American Indians has been based on two sources. The earlier and more pervasive is the information arising through contacts by explorers, traders, travelers, artists, soldiers, missionaries, government agents, and residents of the frontier. The other is the analysis of Indian cultures that anthropologists derive from the evidence provided by Indian informants, artifacts, and archeological remains. Most of our understanding of Indian-white relationship will continue to rest on these two bases.

In *Seeds of Extinction* Professor Bernard W. Sheehan turns to the American leaders in the first decades of the Republic and asks how they perceived the Indian? Clearly these perceptions were not based on intimate contact socially or in business or politics. Two centuries after Jamestown, the Indian frontier had receded from the Atlantic seaboard, and most Americans knew Indians only indirectly.

One method of concept-building about these original Americans was philosophical, and that is what this book pursues. It explores American thought on the origin of man and the first populating of this hemisphere. It asks what effects are attributable to environment. Buffon and others, as Sheehan indicates, posited a deficiency in America which consigned the Indians to "hopeless inferiority," an allegation which Jefferson was at pains to rebut.

From theory the debate moved to the policy options that white America might elect. As here denominated, these were incorporation, manipulation, violence, and disintegration. Incorporation is the code word for assimilation, civilizing, acculturation—the process, sometimes prolonged and painful, by which Europeans became Americans. Manipulation included such options as intimidation, coercion, addicting to "civilized goods," and intermarriage. Intermarriage availed less than might have been expected in "Americanizing" the Indians, because it usually linked a white man and an Indian woman and that usually meant that the children would be brought up in the Indian culture. Incorporation and manipulation thus were nonproductive.

White–Indian relations were marred by recurrent atrocities in both directions, atrocities which exerted an extraordinary fascination on those who wrote on the Indians and through these writings on the American philosophers. Aware also of the Indians' greater susceptibility to diseases brought by the whites and allegedly to alcohol, the philosophers assumed that the Indians were doomed to disintegration if not extinction. Given that frame of reference, even well-meaning friends of the Indian could turn to segregation (removal was their word) whereby the remnant of the eastern Indians could find refuge beyond the settlements where the Great American Desert began and there continue their distinctive way of life.

As in the literature on which it is based, *Seeds of Extinction,* tends to stereotype the Indian more than is accurate, especially Indian culture. The book serves a useful function in analyzing higher, abstract, almost esoteric thinking on the Indians and spelling out the rationalization for removal. By the early nineteenth century mundane forces had already substantially dispossessed the eastern Indians and now pressed for removal.

JOHN CAUGHEY
*University of California at Los Angeles*

**An End to Hierarchy! An End to Competition!: Organizing the Politics and Economics of Survival.** By Frederick Thayer. (New York: New Viewpoints, 1973. Pp. 232. $9.95, cloth; $2.95, paper.)

The business corporation is re-emerging as a legitimate focus of political analysis. Political scientists, who seem to have an uncanny instinct for identifying important political issues after they have become public, are showing renewed interest in the principles which govern the production and distribution of "private" goods and services. We are beginning to shed the illusion that these principles can be studied in isolation from those which inform the allocation of power and privilege in what we still misleadingly label "the political system." Professor Thayer's book contributes to the contemporary American rediscovery, pioneered largely by radical economists, of the once respectable tradition of political economy. Its thesis is the interdependence of the two central principles around which contemporary capitalist democracies organize their governmental and corporate systems, namely competition and hierarchy. These principles are considered obsolete and pernicious: competition, whether conducted by firms or political parties, invariably reduces participation and creates authoritarian institutions, and hierarchial structures, in turn, undermine individual autonomy and foster alienation. The author proposes an alternative organizing principle: structured nonhierarchial social interaction. Its unfelicitous terminology should not distort its simple message: all decisions in society should be made cooperatively, and no one should be in a position to dominate anyone else.

Thayer judges a variety of ideologies and political theorists according to this rather demanding standard: few escape criticism. In barely two hundred pages, we are exposed to the inadequacies not only of liberal democracy, socialism, and capitalism but also of many of the writers who have sought to explicate some of the complexities of these systems. Thayer charges theorists as diverse as Dahl, Schumpeter, Lowi, Wolin, and Marx with still living under the shadow of Thomas Hobbes and Frederick Taylor; they are each accused of denying some aspect of human autonomy. Dahl, for example, is criticized for restricting his advocacy of worker participation to the corporate sector, while Marx's conception of the revolutionary class is attacked because it implies the potential domination of one group by another.

Thayer clearly speaks to a sense of uneasiness about the performance of the institutions of corporate capitalism and liberal democracy which is shared across a wide political and cultural spectrum. The importance he attaches to increased participation reflects the contemporary concern with the relationship between alienation and participation expressed by such recent developments as the reform efforts of the Democraic Party and the interest of corporations in the restructuring of work. Moreover, Thayer has identified an important difficulty for those seeking to democraticize the workplace: competition, even in the context of public ownership, *does* invariably produce inegalitarian relationships in the firm. Efforts to change the governance of the corporation, whether through changes in the method of selection of the board of directors (as Ralph Nader and Campaign GM propose) or via worker self-management (as suggested by G. David Garson and Robert Dahl) will have only a minimal impact on business policies as long as the hegemony of the market remains unchallenged. If enterprises are forced to compete for labor and capital, serious constraints are placed on their ability to transform themselves into republican institutions. Corporate reformers will ultimately have to confront the paradox of a publicly accountable firm in a competitive market system.

Ultimately, however, Thayer reveals more about the inadequacies of much of what passes for social criticism in the United States than he does about the evils of hierarchy and competition. As our political and economic problems become more unmanageable, the urge to find a single explanation to all our difficulties becomes more difficult to resist. This impulse must be especially appealing when the magic formula can be condensed into the title of a book. Like the devotees of the numerous religious cults who are currently making the rounds of our campuses, Thayer contends that he has discovered the key to our salvation; his obsession with the evils of hierarchy and competition dominates the book like a religious chant. No sense of history nor of the limits of human perfectability are allowed to confuse Thayer's argument; the conventional wisdom of politics and economics is dismissed as irrelevant to our future.

Thayer's criticisms of economic and political competition, while not particularly original, are often well argued; what undermines them is their author's disinterest in politics. For all its merciless criticism, *An End to Hierarchy! An End to Competition!* is fundamentally an apolitical book. Those who think that systemic social change entails conflict and discipline—and even disappointment, will find Thayer's observation that, "everything argued herein either exists already or can easily be accomplished" (p. x) reassuring. The possibility of social transformation without political struggle is a recurrent American fantasy; in this Thayer owes as much to Thoreau and Charles Reich as he does to Adam Smith and Ricardo. In his book, Thayer, like many of the more fashionable environmentalists, is reluctant to confront the radical political implications of his analysis.

His refusal to identify the principles of hierarchy and competition with real political interests—an identification which might have made the book a useful one—leads to some confusing observations. Thus Thayer gives McDonalds' Corporation considerable praise for providing its executives with a room equipped with a waterbed, psychedelic lights and stereophonic music at the corporation's headquarters. This room purportedly symbolizes "an emerging organizational revolution" (p. 3) that will transform the American system. To equate this admittedly unusual and imaginative innovation with an increase in worker autonomy and participation is to confuse the style of decision making with its substance. Corporations are increasingly encouraging small-group cooperation and more informal authority relationships at the management level. Yet, it is precisely the "hip" and creative atmosphere at McDonalds' executive headquarters which is responsible for the firm's spectacular success in translating the monotony and boredom of assembly-line production to retail dining. The "unalienated" labor of McDonalds' executives is directly linked to the alienation of the corporation's unskilled employees. That the corporation responsible for successfully marketing Chaplin's automatic eating machine may serve as a model for our future should cause concern, not celebration. Prior to ending competition and hierarchy, we must first move beyond sophomoric analyses of politics and economics.

DAVID VOGEL
*University of California, Berkeley*

**The Politics of Lying: Government Deception, Secrecy, and Power.** By David Wise. (New York: Random House, 1973. Pp. 415. $8.95.)

Deception and politics have always been closely linked in the minds of Americans, and the polls suggest that many people find it hard to tell them apart. Thus, the evidence presented in this book on lying by government officials and organizations will hardly surprise a large part of the public for which it is written. Americans commonly assume that a politician will tell a few small lies and perhaps an occasional big one—as, for example, that he is competent to hold the office he seeks.

As part of their political education Americans may well develop a capacity to discount lying when it takes the form of exaggeration or distortion in party combat. It is hard to remember a recent presidential campaign that has not featured something resembling a "big lie" at the center of what is euphemistically called the political dialogue in the United States. Recall, for example, Kennedy's charge of the "missile gap" in the 1960 campaign or Nixon's promises of "law and order" in his pre-Watergate salad days.

Presumably, the fact that Americans are constantly exposed through advertising to inflated claims for the goods and services they purchase in the market-place helps build some immunity against analogous deception in party politics—not to mention a certain amount of tolerance for the deceit to which they are subjected by politicians.

The question that haunts this book is how to determine when lying is lethal for the norms and institutions of democracy. It is never directly addresssed by the author, David Wise, a Washington journalist who has written extensively on the secrecy apparatus in the United States. At times Wise seems to argue that all lies by political leaders should arouse the same measure of moral indignation. He cites, for example, Lyndon Johnson's spurious claim that his grandfather fought at the Alamo as a prime illustration of the fraud American politicians habitually practice on their constituents. It is, however, possible to take the Johnson speech much less seriously—to look upon it as the kind of harmless boast about the achievements of ancestors in the settlement of the United States in which a great many Americans indulge. Presumably it helps relieve feelings of personal insecurity, though it never seemed to do very much for Johnson in this respect.

Surely, lying of this sort is not on a par with the gross deceptions of modern American politics—those for which Vietnam and Watergate have now become code words. The problem we confront is the task of telling when lying has serious effects, so that we can draw a line between practices of deceit that are consequential for the health of a democratic society and those that are not. If we look to Mr. Wise for help in making distinctions of this sort, we look in vain.

What sorts of distinction might be drawn? One could, for example, argue that the setting in which lying occurs is critical. Surely, lies told in a stump speech on the campaign trail are far less consequential than lies included in an official report by a government agency or a formal address by a President. On the campaign trail some kind of adversary process prevails, and no particular authority is attached to the statements of rival politicians seeking office. An official report, on the other hand, is often based on data solely in the government's possession and carries with it an air of special legitimacy to which a citizen might ordinarily be expected to defer. However, the neatness of this distinction is impaired by the obvious danger of allowing political candidates unlimited latitude to disseminate falsehood about their opponents.

Nor does the author indicate, alternatively, that democratic regimes have sometimes distinguished between lying to foreign adversaries, which is generally felt to be acceptable, and lying to their own citizens, which is not. This distinction—based on the nature of the audience to which lies are directed—has collapsed in modern times, as democratic governments have fallen into the vice of pretending to lie to enemies abroad in order to lie to their own people.

But while Wise does not provide very much help in developing what might be called a general theory of lying, the book does open up some new vistas on government information practices. As organized, the book reveals in considerable detail how practices of secrecy and publicity can be linked together by government officials in a frequently successful effort to manufacture the consent needed to legitimize their power. The first section of the book concentrates on secrecy, and the latter half focuses on techniques of news management. If a major task of the journalist is—as Mencken once put it—to serve as a scout for scholars, then Wise performs his role admirably.

He shows, for example, that the destruction rather than the withholding of data is now the most serious form of information pathology. As a result, the shredder has replaced the classification stamp as the chief instrument and symbol of government concealment. Technology thus keeps pace with the need not only to conceal but also to obliterate records and documents. In 1966, when the Defense Department undertook a review of its classified files, it declassified 710 and destroyed 355,300 documents—a step that some officials must have surely regarded as the "final solution" of the information problems.

Equally interesting—in light of the recent Watergate revelations—is Wise's demonstration that in spite of all the classification procedures available to them, modern presidents still find it necessary to operate a special secrecy system of their own. The growth of secrecy in government has been widely regarded as a product in large measure of bureaucratization. Wise suggests a quite different possibility—that the increasing obsession of the White House with concealment has made bureaucracy the enemy as well as the ally of executive secrecy. In pursuing its private organizational interests by leaking information on presidential plans it dislikes, an executive agency opens the affairs of government to much greater public scrutiny than they would otherwise receive.

Finally, Wise shows very clearly that high government officials often come to regard information they withhold from the public as a form of private property—to be used at their personal discretion and for their own monetary enrichment when they eventually come to publish an account of their career in government. In this way members of the executive elite can derive wealth as well as power from their privileged access to information. "Honest graft" has come a very long way from Plunkitt's day.

If Wise's book does not wrestle successfully with some of the major issues posed by the mendacity of modern democratic government, it excels in the clarity with which it demonstrates how officials can derive both personal and political benefit from their ability to control the flow of information to the public.

FRANCIS E. ROURKE

*The Johns Hopkins University*

**Latin American Political Parties.** By Robert J. Alexander. (New York: Praeger Publishers, 1973. Pp. xxv, 537, $28.50.)

Political scientists familiar with Professor Alexander's work will find this volume, his fourteenth in little more than two decades, his most ambitious effort to date. Aware of Alexander's extraordinary enthusiasm, Frederick Praeger suggested that the author's relatively modest proposal to study the region's radical political parties ("Jacobin Leftists") might be expanded into a descriptive guide to virtually every major Latin American political party, past and present. Alexander obliged: From the *blancos* in Uruguay to the *colorados* in Paraguay, from Chile's Traditional Conservatives to Bolivia's Left Revolutionaries, from the torrid *Movimiento 26 de Julio* to the temperate *febristas,* he discusses three *alianzas,* four *falanges,* thirteen *frentes,* thirty *movimientos,* and no fewer than 249 *partidos.* This is a reference volume—a "discussion of all the important parties of recent decades, as well as the outstanding ones of the period since independence" (p. vii).

Following a brief introduction to the role of parties in Latin American politics, the volume is divided into six parts: personalist and traditional parties (personalist parties, Liberals, Conservatives, and Radicals), the Latin American socialists (in Argentina, Chile, Uruguay and, briefly, in nine other nations plus Puerto Rico), the national revolutionary parties (Peru's APRA, Venezuela's *Acción Democrática,* Costa Rica's *Liberación Nacional,* Bolivia's MNR, Puerto Rico's PPD, Mexico's PRI, Cuba's *auténticos* and *ortodoxos,* and the Dominican Republic's PRD), the Christian Democratic parties (in Chile, Venezuela, and sixteen other nations), totalitarian parties (orthodox Communist parties, schismatic Communist—Trotskyite—parties, the Jacobin left, Castro communism, and fascist parties), and miscellaneous parties (nearly all of the Brazilian parties and the Peronist party and movement). There is no concluding statement.

Alexander's approach is descriptive. He relies heavily upon current periodicals—particularly the *New York Times* and the *Hispanic American Report*—and upon an enviable collection of personal interviews with party leaders. Anyone who has attempted the frustrating task of elite interviewing will marvel at the wealth of data the author has accumulated from talking to party officials during the past thirty years. In no other single volume would we find the responses of Presidents Betancourt, Caldera, Perón, and Prío Socarrás alongside the 1947 opinions of Cleofe Tupac Yupanque de Sáenz, Head of Women and Children's Section of the Secretariat of Indian and Peasant Affairs of the *Partido Aprista Peruano.*

As a compendium of party chronologies, however, Alexander's effort fails to take advantage of the enormous expansion of data on Latin American political parties during the past two decades. It is therefore probable that many students of a particular Latin American party or party system will view Alexander's commentary contentiously, just as I, for one, find criticism of the treatment of Peronism difficult to resist. Although the author warns us in his preface that he is "most disposed toward parties of democratic reform" while studying an area where both Western-style pluralist democracy and Hirschmanian reform-mongering are in increasingly short supply, he also insists that he has "tried to be as objective as possible in dealing with all kinds of parties" (p. vii). Yet the chapter on Peronism reveals clearly that his earlier (1951) appraisal of Perón as a self-serving demagogue successfully hoodwinking millions of unwitting workers has not been altered. This may be due to Alexander's somewhat immodest belief that his earlier effort remains "perhaps the best description of [the] early period of Peronismo" (p. 503), or because the only other sources he consulted were María Flores' 1952 anti-Evita polemic, George Blanksten's competent but dated (1953) analysis of *justicialismo,* a labor union propaganda sheet, an interview with a neo-Peronist labor leader, and the *Hispanic American Report.* Have we learned *nothing* from Smith, Kirkpatrick, Kenworthy, Snow, Díaz Alejandro, Germani, Luna, and a whole generation of younger Argentine scholars led by O'Donnell, Ciria, Cantón, and Catterberg?

In addition to this disconcerting attempt to impose the author's ideological preferences upon an analysis of Argentine political culture and the disregard of two decades of scholarship, a number of factual errors—Frondizi's "impressive majority" (p. 475) in 1958 was 43 per cent of the popular vote; it was Law 13,010 of September, 1947, and not the Peronist Constitution of 1949 which gave Argentine women the same political rights and obligations afforded men; Perón's first vice-president was named Juan Hortensio, not Horacio; Peronists never celebrate June 4th as a party holiday—further detract from the value of his discussion.

Perhaps these are inevitable criticisms of one-person studies of any major Latin American political phenomenon undertaken without the assistance of some unifying theoretical perspective. Moreover, today it is all but impossible to accumulate sufficient expertise to present accurately even a journalistic appraisal of more than 300 political organizations in twenty-one nations. Having attempted just such a task, Professor Alexander must now welcome the opportunity to return to less ambitious investigations and to build upon his valuable studies of Latin American communism, organized labor, and the political systems of Argentina, Bolivia, and Venezuela.

LARS SCHOULTZ

*Miami University*

**The European Administrative Elite.** By John A. Armstrong. (Princeton, N.J.: Princeton University Press, 1973. Pp. xii, 406. $20.00, cloth; $9.75, paper.)

The canons of book reviewing in scholarly journals is as stylized in the 20th century as was the ceremonial at the court of the Sun King in its own days. Ritual requires that each review consist of three parts: an opening section stating the nature of the volume and praising it; a second section, usually beginning with a phrase such as "However, there are serious weaknesses . . ."; and a third and final section introduced with a "Nevertheless . . ." phrase. Musically inclined members of the profession will, of course, immediately recognize this as the classical a–b–a sonata form: statement of the theme(s); development section; and restatement plus coda.

Most books fit this format comforably: there is always something to praise and something to criticize. Occasionally, however, a book comes along which does not permit recourse to the comfortable, albeit by now rather tired, clichés. Professor Armstrong's study of European administrative elites is just such a book. It is exciting, eminently learned, theoretically sophisticated, greatly successful in its historical sweep, yet based on a wealth of sources, traditionally historical as well as quantitative. It is all of this and more; yet it is also opaque in its overall structure; determined to carry an interpretation beyond what the data and alternative interpretations will bear; ending on a note which doubly confounds everything the author has labored to demonstrate in over three hundred pages; and committing itself to a future built on ever more centralized planning and control in the hands of an administrative elite whose qualities of responsibility and responsiveness the author considered not sufficiently significant to explore, except tangentially, an administrative elite whose nearly total history since the seventeenth century, he claims, has been one of shaping affairs to fit its group interests.

In short, it is a book that tries the soul of the reviewer, that challenges him on nearly every page. Yet it is clearly a study that will be required reading for several social sciences and certainly for political scientists devoted to comparative politics and administration, as well as those who try to understand processes of development and modernization. They will want to read it, argue with it, and surely assign it to their students, if only to watch them struggle and agonize in their turn.

The study's central focus, according to its author, is the relation of administrative roles to economic development, or put differently, the role of administrative elites as development interventionists. Its geographic reach is the administrative elites of France, Britain, Russia, and Prussia (adding the German Federal Republic as the contemporary successors of the Prussian administration.) Temporally speaking, the study goes back to the pre-industrial period of these societies, though serious investigation begins with take-off, the author utilizing Rostow's "stages" as the basis for his periodization. However, there is no forward time limit, with contemporary data reaching into the 1960s being utilized and contemporary trends being examined. It is here that a rather serious problem arises. A political science reviewer is most likely to be able to deal with contemporary, or at least post-World War I data with some authority. The present reviewer is no exception, and so he must report considerable uneasiness with the treatment of contemporary developments, especially in Britain, France, and the Federal Republic, which rest on a data base much less solid than the earlier historic periods. To take only one example: Professor Armstrong adjudges the present German elite to be still predominantly legally trained and the dominance of the *Juristen* unbroken (p. 203.) The work was researched in the middle and late 1960s and was written in 1972. I believe that not even then—and certainly not today—could one say that this legal dominance is unbroken. Again, the author concludes that the West German administrative elite does not occupy a central position in the societal elite as once did their Prussian forerunners (p. 213.) After having observed the Federal Republic closely for a year, with attention focussed on the federal administrative elite, this reviewer would judge the situation to be quite different. The senior administrative elite today enjoys financial and social privileges which make it a highly advantaged group with much prestige and visibility. To be sure they do not play a role comparable to that of the *Junker* elite—but then nobody does anymore in the 1970s. Thus by refusing to set a precise time frame for his study, Professor Armstrong has fuzzed the temporal focus of the work and has weakened it unnecessarily. Given the clearly longitudinal approach to the study, nobody could have quarreled with a decision to set a forward time limit at either World War II or soon thereafter.

Though the professed focus of the book is role perception, and specifically development interventionist role perception, I came away from the work with the impression that the author was determined to show how in all four countries the group self-interest of the administrative elite was the sole motive force in shaping everything from family socialization, peer-group socialization, and schooling at all levels through the selection, entry, and career pattern of the higher administrative services. This shifting of the work's focus, of its general tone and argumentation is the result of two fundamental decisions which, to Professor Armstrong's great credit, he has stated clearly at the outset and to which he has adhered scrupulously throughout the study. First, he has adopted a conflict model of social action in preference to a functionalist one (pp. 8, 202); and second, he has had recourse to an ideology/false consciousness explanatory framework, taken from Marx though generally lifted out from the remainder of the Marxian analysis.

It should be said immediately, that it would be difficult, if not impossible, to quarrel with the proposition that the European administrative elites are generally "closed corporations" that arrange matters to suit themselves, largely in defiance of politicians and people; Brian Chapman, among many others, demonstrated that quite convincingly fifteen years ago in his *The Profession of Government: The Public Service in Europe* (London: Macmillan Co., 1959). But where Chapman's essentially descriptive approach (though surely normative disposition!) avoided the constant obtrusion of the basic theoretical points, Professor Armstrong's more theoretically sophisticated and self-consciously coherent analytic stance led him to hammer away at it page after page, mercilessly throwing his false-consciousness floodlight on all objects along the way and thus making them all appear grotesquely alike, all being bathed in the same color medium. The *Rechtsstaat* is merely a device to maintain the *Juristenmonopol;* Whitehall and civil service clubs are bucolic enclaves in metropolitan London; law and classics are useless exercises from a pre-industrial past, akin to the poetry writing of the Chinese mandarins. Does Professor Armstrong really suggest that penal code reform in contemporary West Germany bears "little direct relation to societal patterns" and that the legal framework of that, or any other European society is nothing but a "graceful pattern of words and categories" (p. 173)?

Can everything the administrative elite does really be fully explained by group interest and justified through ideology and false consciousness? For example, in his effort to demonstrate the ideological nature of the role of a classics education on the secondary levels (chap. 7), Professor Armstrong shows (Table XI, p. 139) that in the German classical-type secondary school between 72 per cent and 82 per cent of the curriculum was devoted to humanistic studies during the periods from take-off to the present. This figure is arrived at by categorizing history as a humanistic subject and by including, without special explanation, instruction in the grammar, writing skill, and literature of the native language. But from the post-World War I period onward, at least geography was added to history as a social science block; and another source about this type of curriculum in Germany and Austria presents this distribution: social sciences, 18 per cent; natural sciences, physical sciences and mathematics, 26 per cent; and humanistic studies, 55 per cent (of which 18 per cent is "Deutschunterricht" i.e., grammar, composition, and literature of the native language). Presented in this manner, the curriculum appears much less onesided.

The same sort of ideological treatment is accorded higher education in chapter 8. Again, Great Britain and Germany bear the brunt of the critique here because their higher education for prospective elite administrators (law and the Oxbridge honors program, respectively) reflect "quasi-aristocratic anti-specialist values" that presumably foster "the ability to handle people" (p. 167). Why an ability to handle people is necessarily quasi-aristocratic is not clear; it would seem that any good old self-made American business tycoon would applaud such values. I'm afraid that in pursuit of the conflict model, Professor Armstrong tends to see many connecting links as sufficient ones, when they constitute at best necessary linkage.

The British generalist is seen as based on aristocratic modes that are no longer either valid or legitimate, and the attack on the German *Juristenmonopol* is cut from similar cloth. But at the very end of the study the author makes a plea for a new form of generalist—but a generalist, just the same—one who is trained basically in economics, but one who also appreciates technology, the social sciences, and the humanities (p. 318). It will be this administrative elite to which will be entrusted the ever more highly centralized and planned societies of the future without, seemingly, any concern that all this might again be conducted solely for the group interest of the new elite and that an elaborate ideology might again be constructed to justify this new form of elite domination. This tension between the bulk of the evidence mustered by Professor Armstrong and his concluding glimpse of the future is difficult to accept or come to terms with.

It was said earlier that the structure of the study is opaque; this statement requires some modification. There is no doubt about the purpose and the structuring of the core of the work, chapters 5 through 11, which trace the shaping of these four administrative elites from family through career patterns and prospects. Chapter 12, too, raises no problem. There the author deals with the only specific set of administrative institutions which bear very directly on development—territorial administrative structures. In the two concluding chapters, Professor Armstrong examines two challenges common to the four administrative elites, railroad construction and industrial mobilization for World War I (chapter 13) and skillfully draws out a number of implications of the development interventionist role (chapter 14). These two chapters do not summarize in any conventional sense but nevertheless bring the study to a full and satisfactory conclusion: we see where we have been and what are the consequences of these particular developments. The opening four chapters are less sharply drawn. Both chapters 1 and 2 are essentially definitional—and rightly so. The purpose of chapters 3 and 4 is least clear. The ideas developed there are difficult to follow and seem to lack connection either to what went before or what came afterward. These chapters are tough going intellectually, and it might well be that more concise formulation would have helped.

Touching on a few issues, here and there, can convey only very imperfectly the sweep of Professor Armstrong's argument, the strength of his analysis, and the stimulation the volume has given the reviewer. One is forced to reexamine a whole host of ideas, conceptions, assumptions, and just plain facts; and even if one does not, in the end, accept everything he has to offer, one is nevertheless immensely enriched by the exercise. Indeed, a brilliant and exasperating book; is there any higher praise?

ALFRED DIAMANT

*Indiana University (Bloomington)*

**Colonialism and Underdevelopment: Processes of Political Economic Change in British Honduras.** By Norman Ashcraft. (New York: Teachers College Press, 1973. Pp. 180. $8.50, cloth; $3.95, paper.)

In the approximately two and one-half decades since the question of national political, social, and economic development first became of concern to the students and practitioners of statescraft, conceptual approaches to the problem have practically come full circle. During the first few years after World War II the problem of the colonial areas was largely understood in terms of their political and diplomatic dependence. Break the links of dependency with the mother country (after establishing proper parliamentary and administrative institutions, of course), and the former colonial area would take its proper place in the pantheon of sovereign states.

What was thought at first to be primarily a political and diplomatic problem very quickly was seen to be an economic one. When, however, economic stagnation, balance of payments problems, despotism, military rule and general political and social turbulence failed to give way before the Alliance for Progress, direct international investment, or economic stabilization missions, it was argued that the developmental process involved a complex series of sociological, psychological and cultural transformations. But, now that the United Nation's "Decade of Development" is behind us we are back to the theory of dependency.

In this very interesting study of the political economy of British Honduras, anthropologist Norman Ashcraft brings the unique perspective of his discipline to the study of dependency. By employing social field theory—an "approach which permits investigation and in-depth analysis of people and their behavior at the local level, while at the same time revealing the context of that behavior" (p. 16)—he is able to delineate clearly the interaction between the individual peasant and the communal, national, and international socioeconomic environment. As such the study will be helpful both to the general Latinamericanist and to students of political economy and international economic relations.

Ashcraft portrays the British Honduran peasant as responding rationally to the constraints imposed by that environment, an environment which has its roots in the early development of the colony as an appendage of the developed world. The original economy was based on the provision of forestry products to England and the United States. Since the most efficient way to provision a relatively small population of slaves was with imported food, agriculture was simply a supplementary activity engaged in during spare time on the periphery of the campsite. After the decline of the forestry industry during the 1930s the by then emancipated workers found it difficult successfully to change to agriculture. The addiction of the urban population to buying imported food, the reluctance of urban merchants to handle locally produced fruits and vegetables, and the lack of a well developed marketplace for the local grower all worked together to restrict the market even though the nation has had a perennial food deficit. Consequently, because the peasants could not support themselves through agriculture they felt compelled to take on as much daily wage labor as possible, thereby retarding further their development as viable small farmers. Furthermore, the colonial government encouraged the shift of investment from a declining forestry industry into commercial agriculture which earns much of the foreign exchange needed to import food.

As an anthropologist, Ashcraft sees the perpetuation of milpa agriculture not as archaic or uneconomic, but rather as an efficient means of adapting to an economic situation far beyond the limited capacity of the peasant to control. "There are many farmers who are fully knowledgeable of the merits of permanently cropped fields. . . . They will tell anyone who will listen that changes require heavy expenditures of capital, not only for land improvement, but for equipment as well. They would ask how they could get the increased production to market efficiently. And . . . once they are there, where are they going to find the market?" (p. 170).

Ashcraft concludes by pointing out that underdevelopment in British Honduras is a part of a world system of development/underdevelopment; that it is impossible for small nations whose only marketable resource is agriculture to have any control over the course of their own development. This conclusion may be less valid given the recent awareness by the periphery of its power to withhold resources from the metropolis. Thus the theory of dependency may be around for some time to come, but with a whole new cast of characters.

Roland H. Ebel

*Tulane University*

**Municipal Administration in India.** Edited by A. Awasthi. (Agra, India: Lakshmi Narain Agarwal, 1972. Pp. viii, 539. Rs. 40.00.)

Professor Awasthi has undertaken the herculean task of surveying the state of municipal administration in India. His volume is divided into four parts: (1) description of the problems of urban administration including training in municipal administration, national control and assistance to municipal bodies, and public participation; (2) a survey of municipal administration in sixteen states; (3) analysis of the Bombay, Calcutta, Delhi and Raipur municipal corporations; (4) concluding reflections.

G. Mukharji's statement of the problems of urban government presents no new insight but is rather a compendium of the all-too-familiar ills which beset India's cities. Deva Raj's review of training in municipal administration is a valuable summary of the efforts of Government and the universities to develop training materials and programs to help municipal administrators coping with the mammoth problems of Indian urban life.

Professor Awasthi's essay on "Government Control and Assistance to Municipal Bodies" will aid students of Indian government to understand the relationship between the union and state governments on one hand and municipal bodies on the other. Awasthi traces the early development of such control and assistance in education and revenue promotion, through the broader British objective of promoting responsible government to the present. Widespread arbitrariness of municipal administrators is documented by judicial decisions overturning such actions and by the numerous state supersessions of local administrations for corruption, inefficiency or arbitrariness. Although state governments in India have begun to wrestle with their urban problems, the present collectors and commissioners supervising urban administration still conceive of these problems as secondary to their major statutory duties. Crucial to the understanding of state treatment of municipal problems is the realization that state officials continue to focus their major efforts on the legalistic and fiscal aspect of urban problems rather than on the programmatic ones.

In attacking the problem of public participation in local government, A. P. Awasthi has emphasized such broad cultural factors as poor newspaper coverage of local affairs, low literacy voter rates, lower levels of prestige for involvement in municipal affairs and paternalism in state government. He advocates a three-point program which includes organizational change, the creation of voluntary associations and a broad-based attempt to educate citizens.

The major portion of this volume is a survey of municipal administration in sixteen states and in selected cities. The treatment of the states is similar and includes a historical statement, a description of the structure of municipal government, municipal powers and functions, the extent of municipal services, municipal finance, municipal personnel, and a conclusion section. The reader is thrown into this state-by-state survey without any introduction or guide. As a result, the wealth of material presented is confusing. An introductory section could have described the well-defined municipal models which actually exist. One such model is the Bombay variety, which places executive authority of a Municipal Council in a single chief executive officer. This model is widely applied throughout India. A second model places executive authority in the head of the municipal council; while this model was adapted in Uttar Pradesh, Madhya Pradesh, and Tamilnad, it was abandoned after the conflicts it produced between the municipal council members and the head of the council. A third type is the commission form of municipal government, which was in existence in Calcutta between 1923 and 1951. In a variant of this model, executive power is delegated within the municipal council by means of standing committees as illustrated by the Madhya Pradesh Corporation Act of 1956.

Although a great deal of valuable information is presented in the editor's concluding reflections on the state of administration in India, its effectiveness is lost because it comes too late in the volume. If future editions are contemplated, it would be desirable to have the concluding section precede the survey by states.

The state surveys of municipal administration are completely dehumanized in their presentation. The approach is legalistic, structural, and descriptive. One will search here in vain if he seeks to relate municipal administration to the problems of urban living in India. There is no discussion on the relationship of administration to such vital areas of human life as health and sanitation, housing, transportation, pollution, education and employment. The struggles of the citizens of Bombay as they crowd aboard antiquated buses, the suffocating results of diesel pollution in Delhi, and the failure of educational goals in Calcutta are completely ignored in this volume. From a methodological point of view, the authors have failed to use modern scientific tools in their evaluation. No effort has been undertaken to develop financial yardsticks by which to measure the costs or benefits of service, nor have programs been analyzed in terms of modern management objectives. It is hoped that future scholars of Indian administration can wrest their subject from its traditional moorings to achieve a creative treatment that this book sadly lacks.

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**Mao Tse-tung and Gandhi, Perspectives on Social Transformation.** By Jayantanuja Bandyopadhyaya. (Bombay: Allied Publishers, 1973. Pp. 156. Rs. 24.)

It used to be popular to characterize India and China as engaged in some sort of competition to validate contrasting modes of government by the excellence of their economic performance, this competition presumably to have an effect on what happened in other parts of the world where leaders awaited the verdict. India and China, of course, never were "democracy" and "totalitarianism" personified, and the rest of the world was not waiting with bated breath to see which of these two countries could raise its living standard faster. The entire construct was a cold-war fantasy.

J. Bandyopadhyaya, Professor of International Relations, Jadavpur University, Calcutta, has now attempted to pose the old India-vs.-China question in new, more promising terms. Bandyopadhyaya has broadened the focus, structuring his essay as an attempt to contrast two movements, Chinese Communism and Indian Nationalism. This permits comparisons extending backward in time beyond the formal attainment of political power by these anti-imperialist movements, and including the entire range of issues posed by the social transformations of these two countries. As Bandyopadhyaya shows, useful comparisons can best be made by examining the full sweep of the political experience of India and China from the days of imperial exploitation to the present.

Bandyopadhyaya's perspective makes possible an appreciation of the essentially nationalistic nature of both movements. Both Gandhi and Mao, the author observes, attempted to mute internal class antagonisms to present a united front against imperialism. And while we are unaccustomed to comparisons between Gandhi's technique of nonviolent struggle, Satyagraha, and Mao's strategy of "people's war," Bandyopadhyaya shows that both reflected complex and relatively successful attempts to transform ancient societies thrust into temporary disarray by unexpected Western challenges.

Bandyopadhyaya asks many of the right questions, but his answers are often unsatisfying because his primary concern is to inquire whether Indian nationalism and Chinese communism are, in the author's words, movements "truly conducive to the promotion of liberty, equality and fraternity" (p. 127). Bandyopadhyaya's commitment to those goals he considers ideally preferable hampers his understanding of the dynamic nature of the particular movements he is examining. Bandyopadhyaya, for example, criticizes Gandhi's identification with Hinduism. He describes Gandhi's solution to the Hindu-Muslim problem as "ahistorical and simplistic." He concludes that "the solution of the communal problem in India required a completely secular and multi-pronged offensive, involving the attempt to achieve economic, political, and cultural integration of the two principal religious communities" (p. 89). Gandhi is similarly censured for an indifference to economic development through state initiative. Bandyopadhyaya suggests that once a high level of economic development has been attained, Gandhi's philosophy will become more relevant. "High technology . . . combined with non-violence . . . may yet bring us close to some of the utopias . . . which rare and visionary human beings have dreamt of" (p. 140). A heavy burden of proof must rest on one who contends in this fashion that a great leader misunderstood his situation and that his techniques would be better adapted to some other situation. Gandhi, like Mao, was successful because he worked within a context. Many of Gandhi's seemingly simplistic statements must be seen, not merely as diagnosis, but more importantly, as exhortations forming part of a total program of action. Gandhi and Mao were not scholars seeking objectivity, but leaders attempting to motivate action.

Bandyopadhyaya advocates for underdeveloped countries "a libertarian political system . . . combined with a nonviolent programme of mass mobilization and conflict resolution for the establishment of socio-economic equality, as advocated by Gandhi, as well as with a broad non-Gandhian programme of industrial and technological progress" (pp. 126–127). This is all very well, but suggests the abstractness on which the essay ultimately founders.

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**The Ordeal of Nationhood: A Social Study of India Since Independence, 1947–1970.** By Krishan Bhatia. (New York: Atheneum, 1971. Pp. 390. $10.00.)

Krishan Bhatia, a journalist of long standing, with the experience of covering Gandhi's visits to Delhi, traveling with Nehru and holding top positions in Indian journalism, including editorship of *The Hindustan Times* for some time, has tried in this book what he calls a case study in social dynamics of a mass society in transition. He has attempted the task neither in the style of prophets of gloom who had started spelling out India's dangerous decades when she had hardly set out on her career, nor in the spirit of functional political scientists who try to rationalize everything, but in the sympathetic and balanced way of an accomplished journalist. The narration is impartial and fascinating. Descriptions are detailed and vivid. Criticism is cautious; comments careful, never carping; and appreciation unstinted, but not effusive.

In 1947, India, with a population of 561 millions, was still a colossus, a giant, as Nehru called it, but she was faced with serious challenges to her national unity—in the form of communal differences, language barriers, regionalism, untouchability, and various other political, constitutional and emotional obstacles. Bhatia has not tried to analyze the factors of language, communalism, or parochialism from the angle of a political sociologist, even though he claims to be making a social study of the country. His assessment that, while the disintegrating factors were there, they were not so imminent in the early years has been borne out by subsequent events. Linguistic agitation took a virulent form only in 1953 and led to a redistribution of the provinces in 1956. Hindu-Muslim riots were revived in 1961. Regional parochialism, leading to the rise of Shiv Sena in Maharashtra and other militant organizations elsewhere, is a still more recent phenomenon.

Bhatia has made a good study of the Indian political system and its leadership. Though India's political system was a cumbersome transplant, and an imported model, it made a good start and was working more or less satisfactorily. India has had regular elections, in which the electorate has shown maturity, in spite of 76 per cent illiteracy, and the political processes have been characterized by a high degree of sophistication; changes of prime ministers have taken place more or less smoothly; and political stability uncommon in Asia has established itself. Independence of the judiciary and the existence of a free press have further strengthened the political system. The fact that the real authority rests with the prime minister and not with the president, that the Parliament is a real check on the Cabinet, and that, in spite of there being no challenge to the dominance of the Congress Party, it has behaved on the whole with temperance and restraint, are quoted as examples of the viability of the Indian political system. The author is aware of the danger-signals which could threaten the existence of the political system later on: the planning commission often planning unrealistically and setting targets well beyond financial and physical competence; states, not subject to any authoritarian direction from the center, disregarding priorities and targets; bureaucracy proliferating while its character remains unchanged; the party system new to the country and leading to frequent floor-crossing and toppling of governments; and political parties themselves lacking any political integration.

The Congress Party, "the mother of all political parties," is discussed at some length. The author is aware that the Congress Party, physically wearied even in 1947, had become intellectually and morally exhausted within a decade, but when we see what happened in Pakistan and Burma because of the absence of an effective political party, we can understand the contribution the Congress has made to political stability in India. Bhatia is aware of the party's amorphous character. He mentions Gandhi's advice for the dissolution of the Congress Party without comment, but appreciates Nehru's policy of using its well-rooted national organization for maintaining a stable administration in the country. It is, however, clear, as Bhatia convincingly demonstrates, that the Congress has become increasingly complacent and slothful, it has lost touch with the masses and allowed vested interests, landlords, businessmen, even princes, to creep in; its leaders are ostentatious in living and vulgar in the exercise of power; and they have made ruthless compromises for winning elections. Bhatia has picked up, like a good journalist, some major political events for more detailed comment. He has discussed the Kamaraj Plan in all its implications, including Nehru's alleged desire to use it for the ultimate rise of his daughter as prime minister—a point which he has contradicted by strong arguments—and the 1969 split in the Congress Party, along with the possible alternatives which could have followed it.

His chapters on the Indian army and on India's foreign policy, in its regional and global aspects, offer lucid description and balanced analysis. His discussion of the mistakes that India has made persistently in the field of economic policies especially is thorough and forthright. India's economic development, according to Bhatia, has been exceedingly poor, as a result both of inadequacies of leadership and imperfection of techniques employed. Her land-reforms have been a miserable failure. The top 10 per cent, owning 56 per cent of the cultivable land, and not the small owners or tenants, have received all the advantages of economic development in the country. Nationalization—of aviation, life insurance, road transport and, later, of banks, Bhatia makes it very clear, has been the result not of any deep-rooted commitment to socialistic goals but of political expediency. Where Bhatia seems to have faltered is in analyzing the causes of India's failures, political and economic. His chapters on "Stamina for Survival" and "Values and Attitudes", the weakest in the book, could have been used for a more thorough analysis of the socioeconomic infrastructure from which the Indian political system draws its sustenance. The fact that the book was brought out in 1971 has saved the author partially from the responsibility for this kind of analysis. Developments in India since 1971 have highlighted the strains and stresses, mainly economic, to which the political system is being growingly subjected. India's dangerous decades are not over. They lie ahead!

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**Politics in Rhodesia: White Power in an African State.** By Larry W. Bowman. (Cambridge, Mass.: Harvard University Press, 1973. Pp. 206. $9.50.)

Since its conquest and occupation by Cecil Rhodes's British South Africa Company in 1889, Rhodesia has been ". . . a country where much seems to happen but nothing seems to change" (p. 151). Thus Professor Bowman sets out his fundamental thesis that elements of continuity have largely determined white minority rule in Rhodesia. The author spent 1965–67 as Lecturer in Government at University College of Rhodesia, where he carried out thorough selective interviews and documentary research. The result is a succinct and insightful analysis of the Rhodesian political system in historical perspective.

Bowman focuses on three distinct yet overlapping levels of conflict. The settler/indigenous cleavage, the most critical, is paramount in determining the persisting structural arrangements; the settler/metropole cleavage determines the scope for strategy which can be pursued by the dominant settlers (and conversely the narrow scope confronting any indigenous opposition); and the internal differences within the settler community indicate the success or popularity of any given strategy pursued or set forth by the local government in maintaining domination over the indigenous peoples (or in dealing with metropolitan interests).

In Professor Bowman's view the structure of Rhodesian society was determined by initial British imperial decisions. Since Britain delegated political and economic responsibility for the new Rhodesian colonies to the chartered British South Africa Company, the possible forms of development open to this society were delimited. Given the scope of autonomy ascribed to the chartered company, it was clear that the settlers themselves would be the heirs of that autonomy.

As Colin Leys has previously argued, the sole issue for the settlers has always been the maintenance of power and privilege over the indigenous African population. Bowman thoroughly and effectively amplifies this view in the context of political development after 1958. In addition to his references to the Land Apportionment and Industrial Conciliation Acts of the early 1930s as the foundations of white domination over African land and labor, he also directs our attention to the more recent policies and actions of white Rhodesians which have reinforced their hegemony against the challenge of African nationalism.

In restating that the saliency of race has engendered single-party domination, Bowman also points to the significance of the British regard for the settlers' own preference for internal self-government (as expressed in 1923) as the most important act in Rhodesia's political development. Hence, Rhodesian settler politics have revolved around the strategies and tactics of (1) preserving white rule, (2) expediting political independence, and (3) expanding white privilege. Despite a vastly inferior numerical position vis-à-vis the indigenous peoples (now 250,000/5,250,000), Rhodesian whites have been able to experiment with two distinct strategies in their pursuit of white-dominated independence: (1) the broader framework of the Central African Federation, and (2) the narrow nationalism of a unilateral declaration of independence (UDI).

Perhaps the white Rhodesians' supreme achievement was to overcome British objections to federation with Northern Rhodesia and Nyasaland in 1953-54. Although the settlers proclaimed the theme of "partnership" to the British, their actual goals in federation were "stability + economic development + the British Way of Life." The seemingly moderate United Federal Party's advocacy of this configuration did not meaningfully differ, says Bowman, from the policy goals of the Rhodesian Front ten years later, though the operational base was different. From 1933 to 1962 the UFP was ruled from the top in a paternalistic/autocratic manner (indeed, this style extended into the Rhodesian Front Government of Winston Field). When Ian Smith took power in 1964, a collegial/authoritarian "movement-regime" was established with effective controls emanating from the settler grassroots. Bowman's chapter on the structure of the RF fully elaborates this difference.

On Rhodesian African nationalism, Bowman's assessment is perhaps a corrective to other interpretations. Most observers have been critical of the Rhodesian African nationalist leader, Joshua Nkomo, for not accepting the conditions laid down by the British and white Rhodesians at the 1961 Constitutional Conference in Salisbury. But Bowman concludes that none of it made that much difference. He does indicate, though, that Nkomo and his nationalist colleagues frittered away the one opportunity that they have had to negotiate from a position of at least minimal disadvantage. Bowman argues, however, that this would have brought about the establishment of the same sort of white nationalist party as that of the Rhodesian Front. Further, this entire crisis (for the Africans) was intensified by African nationalist misperceptions of strategy. While they saw the Zambian or Kenyan decolonization models for their solution, they might have benefited more from looking at the Algerian one.

The British, even beyond federation, have consistently inclined toward white Rhodesian rather than African interests. The British objection to UDI has thus been on legal rather than political grounds, i.e., the British have not so much objected to Rhodesian independence under white minority rule as they have to the illegal seizing of such independence by that white minority.

Perhaps there is a comparative dimension which could furnish us with an even fuller understanding of the Rhodesian configuration. From an objective comparative angle, there have been other settler-founded societies which broadly resemble the Rhodesian structure. The author indirectly reminds us of this when he predicts that Rhodesia would probably become either a black state via the Kenyan experience or else more permanently white through the South African one. In terms of the subjective comparative view, white settlers under imperial Britain have expected independence and Britain has done little to discourage this feeling in Rhodesia. Local government (and autonomy) was to the settlers a "British right" guaranteed by the precedent of the Durham Formula in Canada. This point has given them a sense of self-confidence in asserting their own authority over the indigenous peoples and in challenging any British assertions to the contrary.

Professor Bowman's analysis will probably stand as a definitive political history for some time to come. His keen insight and painstaking research have borne much fruit, and it is becoming increasingly difficult to carry out political research in Rhodesia. Certainly Bowman's work ought finally to dispel the myth of British support for democratic government in Rhodesia.

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**Planning and Budgeting in Poor Countries.** By Naomi Caiden and Aaron Wildavsky. (New York: Wiley, 1974. Pp. 371. $14.95.)

"If we were asked to design a mechanism for decisions to maximize every known disability and minimize any possible advantage of poor countries, we could hardly do better than comprehensive, multi-sectoral planning. It calls for . . . information . . . , knowledge, and a political stability . . . undreamed of in their existence" (p. 293). Apply keen insight, ruthless candor, and intellectual chutzpah to the subject of planning and budgeting in poor countries and you get statements like this—and more.

This is an antidotal book, an agnostic book, an incisive and provocative book. It is brazenly intolerant of certain forms of human foolishness. Some people won't like it, and not all of them will be without understandable reasons. Yet this book should be absorbed by students of development, by planners and budgeters in poor countries and would-be practitioners of those arts, by anyone interested in the actual mechanisms of governance in the service of social change.

Much of the voluminous literature of planning addresses intrinsic features of the subject, sometimes in ways that can only inspire awe in ordinary mortals. Likewise, a lot of the literature of budgeting deals with techniques and intents as seen from within the domain of the process. This book looks across boundaries. It examines planning and budgeting in the context of a particular sort of environment—the poor countries. It focuses upon the interplay of process and setting, and shows how the latter influences the effects of the former. The study also treats the interplay of planning and budgeting, and holds that "Budgeting precedes planning. Budgets can be made without long-range plans, but those plans cannot work without budgets. . . . Budget makers can live without controlling the plan, the planners cannot be effective without a say in the budget. When multi-sectoral, comprehensive plans are unrealistic, a few planners are unhappy; when budgets lose touch with reality, a nation is in deep trouble" (p. 290).

From this position the Caiden-Wildavsky book proceeds to argue that one key to enlarged control over the terms of development in poor countries is budgetary reform. National planning in a poor country should, in this view, be merged into the ministry of finance. But "What if people do not feel right without seeing the word 'planning' somewhere on the governmental table of organization? Change the name of the finance ministry by adding 'and planning' to its title. . . . What would become of the planning commission? It would go out of business. Who then would do long-range planning? No one, effectively, as is indeed the case today" (pp. 296–297).

As for budgetary reform, for most poor countries its essence is indicated by the label, "continuous budgeting" (pp. 315–321). A distinguishing feature of budgeting in poor countries is the constant reconsideration of budgetary allocations. Budgets are not really comprehensive statements of governmental intentions for the coming year (p. 316). Uncertainty and the other contextualities of financial management in poor countries allow no way to lick this reality. So let us make the best of it: "let the budget process be explicitly geared to making ad hoc decisions on resource allocation against a background of what is known about revenue and expenditure at the time" (p. 316). Departments would be certain of their established budgetary bases, but variances—additions, new money—would be dealt with continuously, case by case, at any time in the year. The aim would be a better fusion of the opportune and the feasible.

These statements merely sum the conclusions of an argument. The authors begin that argument with a destructive distillation of the rationale of comprehensive planning. They proceed to stipulate the distinguishing properties of poor countries—properties central to any concern with development. If these properties—poverty and uncertainty—are obvious, the authors make clear that their implications for planning and budgeting are not. Three trenchant chapters, reminiscent of Wildavsky's classic work on *The Politics of the Budgetary Process*, portray the realities of budgeting in poor countries. Then the primordial intentionalities of planning are examined—and juxtaposed against the operant realities that Caiden, Wildavsky and their colleagues perceived in field work in fourteen countries and a comprehensive study of the literature. (The bibliography includes about 600 cited items.)

The realities of efforts at comprehensive planning are found to be irreconcilable with the intentionalities. There is no way by which poor countries can really do comprehensive multisector planning. Moreover, trying to do it has more often than not had perverse effects. In the language of the title of Chapter Nine: "Planning is Not the Solution: It's Part of the Problem." "Significant control of the future demands mobilizing knowledge, power, and resources throughout a society. It does no good to propose measures that require nonexistent information, missing resources, and unobtainable consent" (p. 288). Formal national planning is, in essence, not rational.

The evidence incorporated in this study is interesting. This is not a "systematic" study in the sense that some social scientists would insist upon in using the phrase. The method and its defense are presented in the introduction. In its broadest ambit the work covers "over 80 nations and three-quarters of the world's people"—the countries with GNPs of less than $800 per year (p. xii). A dozen of them were chosen as sites for intensive interviewing, and "validation interviews" were conducted by Wildavsky in two others. These data, plus information from reports and publications, provided the basis, not "for a record of contemporary events but to describe how a process works" (pp. xiv–xv). The result is a composite description of the planning and budgeting processes, or the central tendencies in those processes among a large class of countries.

The categories about which information was collected were determined in a straightforward fashion by the concerns of the authors. Those dedicated to comprehensive quantitative planning exercises will challenge the scope and adequacy of the evidence. But the focus here is not upon intrinsic properties of planning technology, but upon "how things work" across the interfaces between planning and its environment, and budgeting and its setting.

The analysis has three elements. One is a description of central tendencies in the processes under scrutiny. This emphasizes the empirical at the expense of pretension. The second is a set of judgments of *why* things tend to work as they appear to. For me, intuition and experience validate the authors' interpretations; others may disagree. Third, the analysis proceeds to praxiology. The proposals derive from a combination of the facts, the explanations, and the wit and expertise of the writers. These include a monumental grasp of the budgetary process, a cogent appreciation of the properties of a certain approach to planning, and a keen political perspective.

It might be argued that to attack comprehensive multisector planning is to flay a dead horse; but there are today a lot of national planning agencies among the poor countries of the world. It can be said that the work lacks a hopeful appreciation of the full potentials of established planning and budgeting technology-cum-ideology. It does indeed. Others may argue whether this is good or bad.

All in all, this book is an epitome of what can be produced when intelligent social scientists address problems—and possible solutions—in instances where important phenomena cannot be reduced to neat determinacy, or subjected to reassuringly computational treatment.

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**The Bulgarian Jews and the Final Solution, 1940–1944.** By Frederick B. Chary. (Pittsburgh, Pa.: The University of Pittsburgh Press, 1972. Pp. 246. $9.95.)

This useful monograph, the first of its kind in English, is an elaborated and somewhat abbreviated version of a doctoral dissertation (University of Pittsburgh in 1968). The problems involving the survival of the Jewish community in Bulgaria proper and the destruction of the Jews of Macedonia and Thrace under Bulgarian dominion during the Second World War, have been the subject of intensive research for a number of years. The more notable published books on this theme include the works by Benjamin Arditti (1962), Haim Kishales (1970), Michael Molho and Joseph Nehama (1965), Natan Grinberg (1961) and Buko Piti (1969), all five of which have been published in Israel, the first three in Hebrew and the latter two in Bulgarian.

Together with the Jews of Denmark and Finland, the Jews of Bulgaria escaped with their lives in the midst of the wholesale catastrophe in Europe. While the Danes were overrun by the Germans and the Finns actively fought on Hitler's side, however, the Bulgarians took an intermediate stand. Lead by their late king, they were Germany's willing junior ally. Bulgaria did not fight the Soviet Union and throughout the war preserved a measurable degree of autonomy vis à vis the German overlords.

The Bulgarians undertook to demonstrate their faithfulness to the maxims of the New Order by harassing, molesting and persecuting the relatively small Jewish community numbering just under fifty thousand. Comprehensive anti-Semitic legislation patterned closely after the German model was duly introduced and enforced. Yet when the time came for the Jews to be handed over to the Germans and deported in the early spring of 1943, Germany's fortunes of war had shifted. Organized opposition to the deportation schemes developed within the government's own majority in parliament. The antigovernment criticism sprang from amongst nominally right-wing and reactionary deputies who thought the deportation plans ill-advised in view of Bulgaria's ultimate national interests. Their outcry, together with protestations from liberal and church elements, blunted the government's will and created a delay which ultimately became irrevocable. While successful in its main design, the spur of protest did not suffice to prevent the deportation and the ultimate extermination of the Jews from those areas in Greece and Yugoslavia given to Bulgaria by Germany as a prize for Bulgaria's wartime collaboration.

The above sequences are retraced by Dr. Chary in some detail. Because of the narrow contours of the monograph, delineated by the subject matter itself, the author should have seen it as his responsibility to supply the reader with a broad analysis of the socio-political dimensions which permeate the story itself. With a few exceptions no such analysis is in evidence.

The relative smallness of the Jewish community and its overwhelmingly urban setting do not suffice to explain why anti-Semitism in Bulgaria did not strike deep roots. The facts are that the Bulgarian Jews did not reach outwards and did not show propensities for assimilation within the majority not because they not permitted to do so, but because they were determined to preserve and fortify their own insular way of life. While participating actively in the economic life of the country and claiming a proportionately larger share of material wealth than their numbers would have justified, ever since the liberation of Bulgaria from the Turks the Jews endeavored to emulate their Christian compatriots in fostering the sentiments of Jewish nationalism. A keen sense of modern Jewish nationalism (primarily of Zionist coloration), augmented by rapid cultural secularization provided the Bulgarian Jewish community with a fully blown autonomous political life of its own. This more than anything else spared the Jews from colliding with the national sentiments of the Bulgarian majority. Except for the cursory outline of the Jewish consistory administrative system Chary makes no effort to depict the inner-political life of the community.

The real workings of a primitive institutional political pluralism preserved by the Bulgarian state during the war and throughout Boris's dictatorship are left amorphous and flat. What were the true motives, or at least the uttered arguments, of those political figures from the right and the left of the parliamentary spectrum who defended the Jews and castigated the official policies of the government? The full texts of their speeches (primarily those of Mushanov, Stainov and Peshev), are readily available, as are the speeches of Professor Tsankov, the principal ideologue of a particular brand of Bulgarian fascism. Yet we are not given even a taste of their political flavor.

Like many others, the author is misled by the demagogue leftism of the Zveno people (p. 4); essentially Zveno was and remained a crypto-fascist group with some enlightened ideas in the economic and foreign policy domains. The influence of Jews in the Bulgarian Communist party was greater than the author believes (p. 32). The most outstanding Marxist economic historian in Bulgaria was a Jew, as was the head of the radio broadcasts from Moscow during the war being beamed to Bulgaria itself. Unfortunately, the author does not make use of the stenographic texts of these same broadcasts (available in seven published volumes) which, indirectly at least, reflect Moscow's attitudes on the problem.

Chary rightfully indicates that "No other institution with comparable influence so consistently opposed the government's anti-Semitic policy as did the Holy Synod of the Bulgarian Orthodox Church" (p. 188). Yet, why did the Bulgarian Church, historically always compliant with the will of the state, behave as it did? The admirable intervention by Angelo Roncalli, the future Pope John the XXIII (apostolic delegate to Turkey and Greece during the war) on behalf of the Bulgarian Jews in the crises of 1943, and the advice given to him by the Vatican not to concern himself with such political matters (p. 188) should have been amplified rather than buried in a footnote.

Chary has done adequate work with the German archives. Yet the author has not tried to master the extraordinary efforts undertaken by the Jewish Agency of Palestine by means of its representatives in Istanbul on behalf of the Bulgarian Jews, even though the archive materials on the subject are available and accessible. It is not for lack of trying by the author that the role of Boris and the events surrounding his death remain unclear. Balan, the king's advisor was not executed by the Communists in 1945 as the author maintains (p. 179). The author is right in pointing out the cruel anomaly created in late 1943 and early 1944 when Bulgarian Jews could obtain exit visas from the Bulgarian authorities while "the major problem for the prospective emigré was obtaining a British entry visa to Palestine" (p. 157); still, the escapees never became "a strong and steady flow" as Chary indicates. Finally, one would have wished that the text of The Law for the Defense of the National Security of 1941, the principal legal instrument of the Bulgarian government's anti-Semitic design, would have been retained in the appendix section of the book the way it is reproduced in the doctoral dissertation version.

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**Israel: Social Structure and Change.** Edited by Michael Curtis and Mordecai S. Chertoff. (New Brunswick, N.J.: Transaction Books, 1973. Pp. 443. $4.95.)

This is a valuable book, one of the few recently published which deals seriously, objectively, and even controversially with some of Israel's complex internal economic, social, and to a lesser extent political problems. The quality of its 27 selections is generally high, though the scope of its coverage is uneven.

The articles deal substantively and extensively with a few key issues, such as Western versus Eastern Jews and the kibbutz. Other issues, however, such as the role of the military, the question of the Arab population, educational reform, the economy generally and the Histadrut in particular, receive less extensive treatment.

The complete exclusion of macropolitics, however, in a highly politicized and centralized state, where economic and social policy decisions are preeminently political in nature and national in scope, is questionable. Admittedly, micropolitics in Israel have received less attention in the past.

In this connection, Myron J. Aronoff's article on development towns bridges the center-periphery gap by identifying the key local centers of power and discerning their ties to national political parties and bureaucracies. Judah Matras explores some of the problems of inducing immigrants to go to and to stay in these far-flung development towns. He cites some of the successful experiments and some of the failures, but leaves unanswered the intriguing question of why some succeed and others don't.

The most important and controversial of the fundamental issues dealt with in this volume is the interethnic question of Ashkenazim (Western Jews) versus Sephardim (Oriental or Middle Eastern Jews), with their concomitant differentials in income, housing, education, occupation, social mobility and status, and political participation, access, and influence.

Two schools of thought on this issue implicitly emerge from among the book's contributors. The first, including Shlomo Avineri, Oded Remba, Yoram Ben-Porath, Moshe Lissak, and Daniel Elazar, may be dubbed the optimists. They tend to view the problem of the Sephardim largely as a consequence of "imported" factors: lower educational levels, limited occupational skills, lack of capital or sources of income from abroad, and a general absence of leadership, compounded by the initial manipulative approach of the government and the political parties.

These authors see a subsequent change in government policy and in the attitudes of the (largely Ashkenazi) political elite, whether out of altruistic, Zionist sentiments or for more pragmatic political reasons. Avineri and Elazar point optimistically to the rise of the Sephardim as a power in local government and party structure. This process is supposed to produce a home-grown political leadership and to lead to the ultimate political and social integration of the Sephardim.

The economic gains made by the Sephardim cited by this school are impressive. Indeed, Ben-Porath (p. 233) finds that the rate of improvement for Sephardim is higher than for Westerners. Given the progress to date, the prognosis is for a further narrowing of the income gap which now puts a Sephardi family's income at about 70 per cent of the Ashkenazi's (Remba, p. 203). Avineri also cites the increased rates of intermarriage (p. 294). Thus, time, elite compassion born of enlightened political self-interest, and the normal political and social processes will eventually lead to Ashkenazi-Sephardi integration.

At the other end of the spectrum stands a smaller, more militant group of writers, including Seymour Martin Lipset, Celia S. Heller, and Henry Toledano. The gist of their argument is that the depressed social and economic situation of the Sephardim, caused by a combination of imported and domestic factors, is now stagnant, if not deteriorating. The Ashkenazi political elite is indifferent at best, hostile at worst. Whatever policies the government has reluctantly adopted, under the pressure of anomic violence, have been cosmetic.

Given the increasing polarization along class and ethnic lines, the only way out for the Sephardim seems to be political, i.e., the organization of a Sephardi political party. Lipset estimates (p. 351) that such a party can "easily secure 20 to 30 per cent of the Kneset seats," and can use the coalition mechanism to achieve Sephardi equality. Why such a Sephardi political party, which has been tried and has failed several times before, would succeed now is left unexplained. And why the cross-cutting cleavages which predispose the Sephardi voter to choose one of the larger, aggregative parties —Labor, Likud, or National Religious—is also unclear.

Neither side in the debate is totally convincing. The real Israel is not a blissfully harmonious social paradise, nor is it a seething class and ethnically split pre-revolutionary state. Moreover, the debate itself is not susceptible to black and white answers. Nevertheless, its relevance, and that of the book, has only been heightened by the 1973 war. On the one hand, the postwar economic squeeze hit hardest those on the lowest economic rungs (mostly Sephardim), possibly increasing their resentment. On the other hand, the end of the post-'67 boom may also put the lid on the conspicuous consumption which, as Howard Pack asserts (p. 189), greatly exacerbated inter-communal tensions. Similarly, while the war may result in a nationalist closing of ranks, the costs thereof may decrease the resources available for the improvement of the Sephardi's condition.

Another chief focus of the book is the kibbutz. With the exception of Bruno Bettelheim's rather naive, idealistic view, the other authors shed some interesting, if not ground-breaking, light on the kibbutz. Editor Curtis's own perceptive, sympathetic, yet realistic article focuses on some of the discrepancies between ideology and reality in today's kibbutz: the necessity of hiring outside labor; the question of privacy and individual freedom versus the interests of the collective; the problems of industrialization —hierarchy, specialization, higher education; the 20 per cent dropout rate and the more general problem of attracting and retaining the youth; while at the same time finding meaningful roles for the aged in a work-oriented community. The decline of the kibbutz in the political system is only briefly touched upon.

Suzanne Keller's analysis of the family in the kibbutz—particularly her critical remarks on the as yet unliberated kibbutz woman—contrast sharply with the rather positive views of Nancy Datan (p. 387) on "the viability of the traditional female role" in Israeli society generally.

On the whole, Curtis and Chertoff have assembled an interesting and variegated selection of articles on some of Israel's chief domestic issues. The book is a useful tool as an introduction to contemporary Israeli society, as background for its internal political problems, and as a reader to accompany a more systematic text in a course on Israel.

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**Les Forces Politiques en Thailande.** By Jean Duffar. (Paris: Presses Universitaires de France, 1972. Pp. 176. F16.)

It is curious that in spite of substantial French influence on law and politics in Thailand, a tradition of archeological and historical research by French scholars on ancient and pre-modern Siam, and continued intellectual and practical interest among the French of Southeast Asia, French scholarly writing about contemporary Thailand is almost nonexistent. M. Jean Duffar of the faculty of Paris University seeks in this volume, *Les Forces Politiques en Thailande,* to close the gap.

The short book (163 pages of text) is divided into two principal parts entitled "The Established Power" and "The Contesting Forces." Within this broad framework M. Duffar attempts to penetrate the dialectic of Thailand's political life and to forecast in a general way the trends of change. Although the book was published in 1972, the material from which the author derived his presentation appeared largely in publications of the mid-'sixties and earlier. He draws entirely on French and English sources, both scholarly and journalistic, and there is no evidence of any work on the ground.

Part One, concerned with the structure of political power in Thailand and its social context, is a sound and comprehensive summary. M. Duffar identifies as central the consolidation of the base of political power in the military and its business based allies. He notes also the fundamental cohesion of the national elite which rules the country within a broadly based attitude that supports its political autonomy and which operates through a comparatively effective administrative apparatus. He also discusses at some length the expanding and increasingly ambivalent role of the United States in relationship to the politics of the kingdom.

In the second part, M. Duffar examines three categories of political forces that he perceives as possibly undermining the establishment. Of these three, the most important, he feels, is the divisiveness and dissidence of minority groups. He concentrates on the insurrectionary groups on the periphery but does not neglect the urban-based Chinese.

The other two categories are examined in the book's most novel chapters—one on "The Economic Menace" and one on "Social Unrest." The author identifies the increasing tension between rapidly increasing population and not so rapidly expanding economy as a trend that clearly threatens the stability of the established powers. The threat has two prongs. One is the possibility that economic hardship will catalyze a reaction between the peripheral insurrection and the bulk of the rural population. The other is the enormous pressure generated on the coherence and respectability of the political class by economic change, extraordinary urban growth (with its attendant corruption, crime and speculation), and, particularly, the appearance of large numbers of discontented and troubled youths (students and otherwise).

Although M. Duffar does not predict the current disarray and perhaps dissolution of the long-standing military core of Thailand's politics precipitated by student demonstrations in 1973, he was able to perceive the possibility some years before the event. Considering the present popularity of the mistaken notion that this student political activity was a complete novelty, M. Duffar's foresight and its evidentiary basis is notable. His more complex analysis—while too brief and not completely free of defects arising from a poor data base—is suggestive and important. His image of a confused force of social unrest, "still groping, consisting of an aggregation of elements, not yet formed and lacking self-consciousness" but with the potential for crystallization, indicates the real complexity of Thailand's politics.

This book is clearly badly needed for informing French readers. For those familiar with the English literature, its merit lies in the thoughtful, although sketchy, analysis.

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**Israel's Political-Military Doctrine: Occasional Papers in International Affairs Number 30.** By Michael I. Handel. (Cambridge, Mass.: Harvard University—Center for International Affairs, July 1973, Pp. x, 101. $3.00.)

"War is the national industry of Prussia," Mirabeau once said. According to graduate student associate at Harvard's Center for International Affairs Michael Handel, "this aphorism aptly describes Israel's situation; the questions of war and survival are the main preoccupations of the nation" (p. 69). After setting out the limitations of geography, demography, economic infrastructure, and political environment, the author traces Israel's military development from infantry based strategy to reliance on the most modern tanks, armor, and air force.

Published only three months before Arab-Israel war number 4 in October 1973, the monograph makes possible some timely insights into limitations of the country's political-military doctrine. Until the Yom Kippur War Israel's greatest strategic asset seemed to have been a capacity quickly to innovate and adapt to requirements of rapidly altering military situations. Israel's strategists, however, seemed to have lost some of the flexibility and adaptability with which Handel credits them. He states that IDF practice had been to assume "that the next war will and must be fought very differently from the last one," (p. 70), paying attention to negative lessons such as "what was not relevant for the next war." The political crisis that followed the October War indicated that the public was dissatisfied with performance of their military leaders and that lessons supposedly learned from the 1967 war were not adequately applied by 1973.

Although Handel writes respectfully of former Defense Minister Moshe Dayan's contributions, he is critical of the ex-general's "little understanding of armored warfare" (p. 26) during the 1950s and his lack of creativity and imagination in the conduct of reprisal raids (p. 35). Perhaps a harbinger of Dayan's recent political difficulties was the debate within the Israel Defense Forces (IDF) after 1967 about whether to erect defensive lines against Egypt along the eastern bank of the Suez Canal. The resulting "Bar Lev Line" was supposed to be a forward position manned by few soldiers with the main defense assigned to mobile armored units. "Many senior officers argued that it made little military sense to put the first line under the superior artillery firepower of the Egyptians, and that a better solution would be to build the same first line of defense along the mountain ridge some fifteen miles east of the canal" (p. 51).

Analysis of the country's military doctrine is compact, intelligible and comprehensive. But the account of Israel's political perspectives indicates that the country's leaders have been so preoccupied with immediate security requirements that they have failed to realize the long term implications of war as the national industry. The "basic assumption" of the whole political-military doctrine is that "the central aim of Arab countries is to destroy the state of Israel whenever they feel able to do so" (p. 64). While peace is desired, security is more important, the author writes.

It must be obvious, even if these basic assumptions are correct, that an efficient military machine, if necessary, is far from sufficient to assure the country's longevity. None of the other components of the doctrine presented including alliance with a strong power, developing capacity to produce its own weapons and to become self sufficient, large scale immigration of and support from world Jewry, or continued preemptive strikes against potential Arab attacks, can guarantee the country's security indefinitely. Although the gap in economic and social development with its Arab neighbors may continue to widen, thus assuring the Jewish State another few decades of military supremacy, one wonders whether a nation in Israel's perilous circumstances can afford to place such confidence in the perpetual weakness of hostile neighbors. The present political-military doctrine is shortsighted in giving so little thought to the volatile shifting of international alliances, the decline and fall of empires, the sudden ascent and descent of superpowers. In a world of such rapidly changing power factors no small nation can rely on its own power to survive in isolation. Even development of nuclear arms is not reassuring, for as Handel states, "There is little comfort in retaliating after one's country is already destroyed by one or two nuclear bombs" (pp. 48–49). Many Israeli leaders insist that they have no alternative, (*ain breira*) to the doctrine so well described by Handel. Others insist that if it is worth surviving at all, alternatives must be found which diminish dependence on military and accentuate the development of political components in national security.

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**The Communist Tide in Latin America: A Selected Treatment.** Edited by Donald L. Herman. (Austin: The University of Texas Press, 1973. Pp. 215. $7.50.)

This collection of four essays and an introduction is mistitled. "Communist Outposts and Sallies in Latin America" would be a better description of its contents. Robert J. Alexander's "Impact of the Sino-Soviet Split on Latin American Communism" offers the reader more than the title promises. Alexander draws on an extensive and intimate knowledge of leftist politics in Latin America to provide a cogent explanation of developments which would have eluded him if he had confined himself to communist doctrinal formulations. For example, he makes much of the factor of generational political attitudes in explaining Venezuelan events. He sees the major split in the Latin American left as between the Jacobin Left exemplified by Castro and the other forces on the left including the Communists. Alexander, who first applied the term Jacobin to this movement, understands it as a nationalistic, xenophobic, and above all activist movement drawing its shifting membership from high-school and university students and from young professionals. In Latin American social conditions such a group is inevitably drawn from the middle and upper classes. In Venezuela these young people loosely organized in the MIR (The Left Revolutionary Movement) exercised a fateful and fatal attraction on the Communist Party. The Communist Youth, who had more in common with their contemporaries in the MIR than with the aged leadership of their own party, foisted a policy of armed resistance on the party. Armed resistance consisted mainly of urban terrorism whose chief victims were the working classes—theoretically the mass base for the Communist party. By 1963, Alexander tells us, revolutionary activity had aroused so much distaste in the population that Betancourt was able to deprive the revolutionary members of the Congress of their parliamentary immunity and go on to win a smashing victory at the polls in which almost 95 per cent of the electorate voted. The Jacobin Left was the victim of its own misreading of the Cuban revolution. Violence had succeeded in Cuba, as Guevara pointed out in his early writings on the guerrilla movement, because the Batista government had bankrupted itself, and the Cuban middle classes and to a lesser extent the Cuban workers were sympathetic to Castro. This single episode exemplifies the cogency of Alexander's analyses, based as they are on an intimate understanding of the forces of Latin American politics rather than on a stereotype of how Communist parties behave.

J. Gregory Oswald's "Introduction to USSR Relations with Mexico, Uruguay, and Cuba" is descriptive and traverses familiar territory, but Donald L. Herman's "The Left Wing and the Communists in Mexico" deserves more extended comment: it deals with the influence of the Communists in the Cardenas administration of 1934–40. Herman's purpose is to examine the proposition that "dynamic liberal movements, or native social revolutionary movements, under the guidance of democratic left wing political parties, can offer the strongest resistance to the influence of Communism." He finds that theory inapplicable in this case study because the Mexican Communist party grew from 17,000 in 1935 to 33,000 in 1939, according to Communist statistics. Communists were able to occupy the highest positions in the Mexican Confederation of Workers, a popular-front organization which the Communists were instrumental in creating. The Communists in the Secretariat of Education were able to produce and distribute popular textbooks and pamphlets which explained the class struggle in Marxist terms. But the reader wonders if all these unquestionable facts do not prove the opposite of what Herman intends, suggesting instead that Cardenas exploited the Communists for his own purposes, using them to bring the labor movement firmly under government control. It was an unequal struggle. As the extensive literature on the subject makes clear, for all its disappointments, the Mexican Revolution had earned the respect and affection of large elements of the population. Its army was under the control of revolutionary generals; it presided over the industrialization and the urbanization of the country and during Cardenas's administration it established the precedent for all Latin America of the successful nationalization of foreign owned property. Against these solid achievements the Marxist pamphlets of the Ministry of Education weighed only slightly in the balance. Latin American political elites have always been hospitable to foreign ideologies, but the promiscuity of the hospitality suggests an underlying confidence that verbal formulas are only the outer garments of the real body of power. To change the metaphor, communism, rather than being a tide which engulfs the landscape, seems to be a rivulet which runs dry in the sands of Latin American politics.

The obvious exception to this generalization is the adoption of communism by the Castro regime, which is probably more readily explained by an examination of Cuban, Soviet, and U.S. politics in 1961 and 1962 rather than by the strength of the Communist tide.

John W. F. Dulles's "The Brazilian Left: Efforts at Recovery, 1964–1970" is of particular interest because Dulles is able to document from captured diaries and notes the irresponsibility and stupidity of the Brazilian communists and the fecklessness of Chinese and

Cuban agents. One often suspects that Communist revolutionary leaders are endowed with neither common sense nor a firm grasp of reality, but extensive documentation for that suspicion is rare. Perhaps, however, one should not judge the Brazilian communist leadership too harshly because their situation was basically unimprovable. A Brazilian poll showed that the highest percentage of those who feared communism (81 per cent) were illiterates, and the lowest (41 per cent) were university graduates. The lower classes of society feared communism, and the educated who feared it less were more susceptible to the appeals of the right wing in Brazil. In our distaste for repressive regimes we often forget that they can promote dramatic economic expansion and concomitant social mobility; the chief beneficiaries of this process are the educated, who in conditions of stagnation find radical solutions and conspiratorial life attractive. When careers are open to talents and savage repression is visited on those who spurn the offer, Communist parties fare badly.

*The Communist Tide in Latin America* demonstrates once more that to understand Communist movements in Latin America it is more useful to look through the end of the telescope marked "the political process in Latin America" than the end marked "what Moscow wants."

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**From Villiage to State in Tanzania: The Politics of Rural Development.** By Clyde R. Ingle. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 279. $12.50.)

Ingle's book is a good analysis of the politics of rural development in Tanzania, an African country many observers feel is one of the continent's most intriguing political experiments. The Tanzanian emphasis on the development of the rural sector is unique in Africa. Other countries pay lip service to the importance of the rural sector, but only Tanzania—in a continent where eighty per cent of the population are incredibly poor subsistence farmers—appears to place development dollars alongside rhetoric. Professor Ingle is interested in the relationship between the ideals of the Arusha Declaration, which set forth Tanzania's goals as well as the methods of implementing them, and the behavior of peasants in rural areas. Like others trained at Syracuse, he is concerned with linkage. What makes his treatment of the Tanzanian material different is that he does not stop with juxtaposing ideology at the top and practice at the bottom.

Ingle's theoretical approach is based on systems analysis. But rather than take the more common assumption of those concerned with the state and ask how the state maintains itself under stress, Ingle suggests that in developing countries "systems lower than the national may also develop to counter stresses and demands upon them, the principal source of which may be the national system itself. In so doing they may jeopardize the development of the longer systems of which they are a part" (p. 19–20). Ingle puts forward three propositions which elaborate the foregoing position and guide his inquiry. He suggests that "within any national society the political functions of that society are shared by a series of political systems," that "political actors occupy political roles in the several political systems in the same fashion as they simultaneously occupy several roles within social systems," and finally that "an input-output exchange occurs between the several political systems in a national setting" (p. 23).

Ingle takes us to the local level of Tanzania politics—to the level where both the Tanzaniophiles (to use Ali Mazrui's word) and the regime's sharpest critics say the action is, but where previous authors have not taken us. In *From Village to State in Tanzania* the author is able to show the interface between village and state in a way that other commentators on Tanzania have not done. Liebenow's *Colonial Rule and Political Development in Tanzania* (Evanston: Northwestern University Press, 1970) for example is limited to a much earlier time period. Bienen's *Tanzania* (Princeton: Princeton University Press, 1970) focuses on the national level. Hyden's *TANU Yajenga Nchi* (Lund: Uniskol, 1968) lacks theoretical underpinnings and a synthesis of observations.

There is a good chapter on rural development during the German and British colonial periods, followed by a very insightful chapter on the environment of development in its physical and cultural dimensions. Ingle is cognizant of how political culture conditions development efforts. He discusses witchcraft, paternalism, and compulsion as they relate to rural development. He does not take the "oh-aren't-they-unusual" tone but instead offers realistic appraisal of how cultural milieu constrains efforts at change.

Ingle's analysis of district-level politics and their relationships up to the national level and down to the village level provide a penetrating look at the dynamics of rural politics. The author shows how individuals and institutions react when intermediate political systems attempt to implement policies which require changes in behavior that people are neither enthusiastic about nor willing to impose openly. He suggests that accommodation to demands is the most frequent and minimal response. When greater coercion or will are present, acquiescence is the response. Withdrawal is yet a third possible response to attempts to change behavior. Ingle concludes his assessment of rural politics by recognizing the paradox facing Tanzania's leadership. They took the revolutionary step of decentralizing political authority in order to assess national-local conflicts while at the same time tightening the national control over the extraction of resources. His prediction for Tanzania is cautiously optimistic.

This book should help to satisfy the continuing need of many who teach courses in African politics, in comparative politics in developing areas, and in ideology, for a good middle-range theoretical treatment of the issues raised by experience in a single country.

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**Political Development: A General Theory and a Latin American Case Study.** By Helio Jaguaribe. (New York: Harper & Row, Publishers, 1973. Pp. 603. $11.95.)

This book is fundamentally a product of Professor Jaguaribe's long-time interest in the drama of development, and of his concern for the special role of intellectuals in it. It proceeds from the belief that political science now possesses the tools to fashion an "objectively optimal, or at least particularly fitting, recipe for the promotion . . . of the political and overall development" of a national society (p. 2). This is a book meant to be "policy relevant" at the grandest level.

Its design reflects the scope of that ambition. The first two "books" of the massive tome are essentially critical surveys of existing theory in political science, out of which Jaguaribe delineates his own views. The first is intended to establish the distinctive functions of the political in human society, as the foundation for discussing, in the second, what would constitute political "development." The third and concluding "book" is a case study of Latin America, which is to demonstrate the utility of the broader scheme.

Thus conceived, the work claims originality —but very self-consciously from within a tradition. Jaguaribe shows an Encyclopedist's enthusiasm for the advances of political science in the last decades. He marshals 2500 years of theorists (40 pages of bibliography, in seven languages) to be codified, with some casuistry, within systems-functional categories. In the manner of comparative politics in the '60s, classification abounds. There are literally 100 pages of charts, tables, typologies, and schemata—laid out with a sense of excitement at the accumulation of human knowledge. The result, at this level, is at the least an analytical and often provocative overview of theorists who have profoundly shaped the discipline as practiced—from Parsons, Easton, and Apter, to Huntington, Eisenstadt, and Almond.

Beyond this labor of conglomeration and clarification, Jaguaribe has enriched the concept of "political development" with several distinctive elements of his own. Like earlier theorists, he strongly believes that development is an enterprise of the nation-state. Unlike them, he does not believe it can be understood (or fostered) with the implicit assumption of national autonomy. (That, in some form, is the goal, not the starting point.) He attempts systematically to include international factors, ranging from geopolitical considerations (e.g. "national viability") to economic and cultural influences (from varied kinds of "imperialism" and "dependency"). He quite defensibly devotes a large portion of his study of Latin America to an analysis of the United States, both in its internal development and its external behavior (with George Liska given special prominence). His analytical tools are vague about mechanisms and processes, and are certainly less powerful than he believes (he is one among many who failed to anticipate the "structural" shift of worldwide primary-product inflation). But he does persuade us of his central point, that comparative politics must include study of external influences and interactions.

Another distinctive emphasis, very much in keeping with his larger "policy" purposes, is his insistence on the inevitable intermingling in development of the economic and the political. (Here and more generally Jaguaribe's concerns echo the earlier work of such different scholars as Barrington Moore and Charles W. Anderson, neither of whom, curiously, he seems to have read.) "Political development," if it occurs at all, is inescapably shaped both by economic demands and interests and by the strategies and goals adopted by elites to achieve economic development. It is for the latter that Jaguaribe, building on his own earlier work, elaborates three basic "models" of political and economic development—national capitalism, state capitalism, and developmental socialism. Each of them constitutes a path consciously to be chosen by national elites—considering their own case within the scheme of conditions, categories, processes, and parameters that Jaguaribe has laid down.

He cannot be said really to have succeeded in this task, his aspiration for policy relevance. The reasons for his failure, however, owe not so much to his own shortcomings as to those of his school. The implicit determinism built into doggedly "structural" thinking leaves no room for politics. Here Jaguaribe's key section on elites, which potentially could link structural and choice perspectives, is a great disappointment. It is not very helpful to invoke "political genius" for developmental success; nor to explain failure in terms of a lack of "men of high quality," or of "sufficient support" or of "appropriate leadership." Jaguaribe does not much tell us *how* leadership which is "semi-functional," "non-functional," or "dysfunctional" can *become* "functional" to his "objective" model-recipes (pp. 264ff). (A more systematic development of his many interesting asides about intellectuals would be a good start.) In short, he gives us little insight into what leaders *do* with what they have, after they know what they face.

But even to know that—the resources and constraints for development in a given case—is ultimately nothing for which Jaguaribe's elaborate machinery is of much use. His case studies of Latin America are thin and descriptive, lacking in comparative rigor and persuasive detail. Where they are illuminating, their insights often have little to do with the larger conceptual scheme (as, for example, on the dynamics of military regimes). Jaguaribe suffers here from the same problem as earlier scholars (e.g., Black and Chalmers Johnson): however useful their classificatory grids to frame paradigms for display, they don't tell us much about *particular cases*. However subtle and sophisticated their conceptual distinctions, we never seem to be able to operationalize key terms or specify parameters well enough to make them relevant to ongoing policy or politics. Historically, the greatest impulses to development seem to have come not from cross-sectional comparison but from longitudinal reflection—from the effort, often idiosyncratic and parochial, to grapple with a nation's own past.

Jaguaribe's *Political Development* is rather like Neuschwanstein, the grand, anachronistic castle of Ludwig II of Bavaria—a monument to the style, strengths, weaknesses, and excesses of an idea and its times. It is also a "transitional" work (to borrow a phrase Huntington applied to himself some years ago). Scattered through its pages are concepts such as "the state," "corporatism," "regime," "authoritarian," "dependency," "intelligentsia"—none of which were prominent in the comparative theory of the recent past. They seem to arise from politics as it is experienced and understood in the area Jaguaribe knows best (one which had little impact on comparativists earlier). Ironically, they appear now among the fruits of that unprecedented outpouring of research abroad which was so much stimulated by the older grand theories. As such they can perhaps be taken as signs of a movement already underway to work our way back from our greater knowledge toward new theory, perhaps more modest, hopefully more useful, certainly more solidly based.

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**The Transformation of Communist Ideology: The Yugoslav Case, 1945–1953.** By A. Ross Johnson. (Cambridge, Mass.: The MIT Press, 1972. Pp. viii, 269. $15.00.)

In addition to having written a useful book on Yugoslav Communist ideology, Ross Johnson is to be congratulated for paying attention to the importance of Communist ideology generally.

Ideology is seldom a blueprint for actions of Communist Party states, but it is an indispensable background for understanding what they do and do not do. Failure to realize this is one reason why Western commentators on communism are so often wrong. The American reaction to Yugoslavia's dispute with Moscow—although Professor Johnson does not refer to it—is a good illustration. Once the rift was taken seriously here, there was a tendency on the one hand to expect Yugoslavia to join the United States in its Cold War stance against the U.S.S.R.—e.g., by affiliating with NATO—and on the other hand to fret about Tito "going back" to the Soviet Union. There was no likelihood whatsoever of either of these courses of action, as any competent student of Yugoslav ideology could have testified (and some did).

The emergence of a peculiar Yugoslav version of Communist ideology has been one of the most significant developments in Marxism since the Bolshevik Revolution. This book reviews the Yugoslav ideological positions as they existed before the 1948 break with Moscow, traces their evolution in following years and analyzes the factors, domestic and foreign, which led to the dramatic changes. In so doing, the author makes clear the role of ideology in Communist party states generally and in Yugoslavia in particular.

It is no criticism of the author to point out that he has encountered the problem which has confronted virtually every scholarly writer on Yugoslavia in the past 20 years: the constantly changing political and economic kaleidoscope of the country tends to make all publications somewhat dated. The history of Yugoslavia in this period has been like a pendulum, especially—though not only—as concerns the role of the Communist Party. Calls for democratization were followed by demands for tighter controls, which in turn were superseded by more decentralization. Dr. Johnson quite rightly indicates that only after the purge of Ranković in 1966 was there really an effective departure from the idea of democratic centralism, which the Sixth Party Congress in 1952 seemed to herald. But today, 22 years after the Sixth Congress and eight years after the Ranković affair, this is still—along with the nationalities problem—the hottest issue in Yugoslavia. Tito has called for a revision of the 1952 Party Congress and has purged the "best and the brightest" of the younger Party leaders in the republics, who resist it. Whether he can succeed, and if he does whether success can be compatible with "'Titoism" as it has developed up to now, are questions which involve the future not only of the system but, perhaps, of the country itself.

In addition, there is now still another constitution—the fourth since 1946—with further new concepts, especially the "delegate system" of representation which, it is now claimed, is "basic" to the Yugoslav idea of "withering away" of the state. None of these developments, of course, gainsay the essence of what Dr. Johnson has written, although they place certain aspects in a somewhat different perspective.

The lacuna of discussion about the relationship between "withering away" of the Party and "withering away" of the state is the Yugoslavs', not the author's. Some might feel, however, that the Yugoslav doctrine of "withering away of the state" deserves more elaborate treatment than given in the book, especially since it involves one of the central themes of Marxism, surplus value. The Yugoslav position, first advanced publicly by Djilas before his fall from grace, is that surplus value inheres in socialism as well as in capitalism. The essential question is not who owns the means of production but who controls the surplus profit. The Yugoslav answer to this question was worker management—later expanded to "social self-management" generally—and this in turn required dismantling of the state apparatus. Thus, as Edvard Kardelj formulated the concept, a state is not really socialist unless it is "in the process of withering away."

If Dr. Johnson, whose use of original sources is prodigious, had referred to what I, at least, regard as the most authoritative Yugoslav ideological work in the period preceding the Sixth Congress—Kardelj's *Socijalistička Demokratija*—these matters might have been covered.

One of Johnson's most innovative points is an emphasis on differences between the Soviet and the Yugoslav conceptions of "people's democracy" prior to 1948. In my opinion, however, the situation was more complex than he indicates. There certainly were differences, but the Yugoslavs did not then recognize them. The differences were objective, stemming from the nature of the Yugoslav revolution. Prior to the 1950s, there was no formal Yugoslav party doctrine, *qua* doctrine, on this subject. The initial Yugoslav approach to socialism was marked more by enthusiasm and confusion than doctrinal logic. Yugoslavia was then predominantly a peasant country, and the Yugoslav Communists—backed by the broadly based Partisan movement—perforce had to rely heavily on peasant support. This accounted in part for the early nonrevolutionary treatment of the peasants. On the other hand, the Yugoslav five-year plan was based on the idea they could build socialism immediately. From the viewpoint of Soviet Communism, Stalin was quite correct in looking askance at this approach. Wholly aside from the ideological questions it raised, practically it had no chance of success—at least without vast aid from the USSR, which was unavailable even if Moscow had desired to grant it. Then, in late 1947, the Soviet conception changed, calling for a more doctrinaire approach toward the countryside, i.e., rapid forced collectivization. This was exactly what the Yugoslav Communists could not do, because it would have undercut much of their support.

Alienation of the peasants was not a major issue for the other Eastern European régimes, however, because, unlike Yugoslavia, the Red Army was there to keep things in hand. How confused the Yugoslav Communists were about all this is seen by the fact that *after* the break with the Cominform they belatedly tried to enforce Soviet-style collectivization, with near-disastrous results. Not all Soviet criticism of Yugoslavia was without ideological foundation.

Over-all, the Johnson book is a first-rate scholarly contribution, both objective and knowledgeable, with many original insights. It throws light not only on Yugoslav developments and Communist ideology but also on the whole relationship of theory and political action.

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**The Politics of Ceylon (Sri Lanka).** By Robert N. Kearney. (Ithaca, N.Y.: Cornell University Press, 1973. Pp. 249. $13.50.)

Robert Kearney's *The Politics of Ceylon (Sri Lanka)* is a welcome addition to the growing bibliography on Ceylonese government and politics since Independence. In the series on South Asian Political Systems edited by Richard Park, it mercifully spares us the neologisms and esoteric conceptualizations that were fashionable a few years ago. Professor Kearney makes an orderly argument, his language is straightforward, and he brings to his study detailed knowledge from a number of research stays on the island. In compact compass, the book represents a first-rate introduction to the political system as we conventionally define it.

The author's implicit political model stresses the articulation of demands and their aggregation through political parties which compete in elections to place their principals into highest office. For most of the book, as for Ceylonese politics itself, short-run political power is the name of the game. The last chapter, however, does consider "Challenges to the Political System" generated within the system itself and by the stagnating economy.

A brief discussion of Ceylon's society and history is followed by a description of the structure and process of government, focusing on the legislature, the Cabinet, the public service and administration, and on local government. The contrasting and parallel orientations, structures and memberships of the political parties seeking to survive and win in a competitive pluralistic setting are analyzed with sensitivity. Kearney's descriptions of elections, voting, and campaigning evoke the atmosphere and process of Ceylon's elections which have so well institutionalized government changes that no government since 1956 has yet succeeded itself. Each of these parts of the political system is neatly characterized, its evolution traced and some of its typical processes identified.

Chapter 5 lays greater stress on the more uniquely Ceylonese characteristics of the system. Devoted to "Communal Loyalties and Groups in Politics," it illuminates the underlying religious, linguistic, and caste identifications which still form the substructure of all political behavior. It is from these political elements that leaders put together their winning (or losing) coalitions to contest elections. For readers new to South Asian politics, this chapter is a good introduction to the social springs of political action. One wishes the fascinating materials in the chapter had been used from the outset to show the reader the very foundations on which the system thus far has rested, rather than appearing only after elections have been discussed. But the discussion itself is systematic, balanced and illuminating.

Thus far, the study is generally up-beat. Kearney shows (a) how the system induces the politically ambitious to respond to mass aspirations, at least in the Sinhalese community; (b) how it promotes vertical integration as the rural mass Sinhalese voter affects the way the Sinhalese political elite lives, acts and allocates; and (c) how it regularly brings down incumbents, to give others a chance to rule. He believes that voting by interest is increasing while more traditional voting by deference and patron-client relationships is declining. The changing composition of Parliament and the public services shows that even though the two clan-like families, the Senanayakes and Bandaranaikes, have monopolized the prime ministerial position since Independence, substantial social changes within the Sinhalese community are reflected in changes in the personnel of successive cabinets and other positions in the political and governmental system. He also notes the increase of direct political influence on the bureaucracy and the decreasing opportunities open to the hitherto much more prominent Tamil community.

Chapter 6, by contrast, elaborates the challenges besetting the system, including the problem of coercive protest, the political violence of the youth rebellion in 1971 and the near-stagnation of the economy. Alas, the latter problem has grown increasingly serious since terms of trade began to slip in the mid-1950s. Terms of trade have deteriorated more regularly than for most plantation economies, and the economy has shown little capacity to diversify or to concentrate on new productive activities. These handicaps make it increasingly difficult for the government to meet the demands that are intensified by the competitive political system itself. Each successive government's efforts to do so has reduced the resources available for innovative investment designed to diversify the economy and to lay the base for new sectors of economic growth.

While it is gratuitous for a reviewer to sketch the book he wishes the author had written, this study illustrates a wider problem in our comparative politics literature. Too often fascination with competitive group interests and the growth of political ideas and institutions leads us to ignore underlying economic trends. In Sri Lanka, growing economic cramp has been quite as crucial a limiting factor in the political process as the important forces described in Chapter 1 on Ceylon's history and Chapter 6 on its communal and ethnic structure. Significantly, the economic conditions are both "input" to and "output" from the political system as it has been functioning, representing an instance of cumulative circular causation in Myrdal's phrase. Competitive politics have virtually forced extensive distributive policies, which have clearly impeded either private or public productive investment. With a rapidly growing population this in turn perpetuates and intensifies economic cramp. Accordingly, the nexus between political activity and economic performance in countries like Ceylon would seem to justify more than the ten pages that Kearney devotes to it. Perhaps future studies in this useful series will pay more attention than their predecessors to the economic setting of politics, the economic consequences of political processes and the long-run political effects of persisting economic stagnation in the face of growing population and rising expectations.

But Kearney's volume is a valuable and shapely introduction to the politics of Sri Lanka. It should be particularly useful in introductory courses in South Asian or Comparative Politics.

W. HOWARD WRIGGINS
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**Modernization, Dislocation, and Aprismo: Origins of the Peruvian Aprista Party, 1870–1932.** By Peter F. Klaren. (Austin: The University of Texas Press, 1973. Pp. 189. $8.95.)

This compact monograph by a historian makes interesting reading for political scientists. Not only is it an impressively researched, well-written interpretation of the origins of Latin America's most durable indigenous reform party, but the author frames his analysis in terms of the dual processes of socio-economic modernization and political development. In doing so he helps clear up certain anomalies of Peruvian politics while at the same time deepening our understanding of the political implications of modernization. Why did the Aprista party emerge in Peru and how does it manage to persist? In dealing with this question, Professor Klaren gives us new perspective on the Peruvian military, which took a reformist tack in 1968. While his use of social science techniques is quite limited, Klaren explicitly approaches the specifics of the Peruvian case in such a manner as to give the reader new insight into the dynamics and causal interplay of modernization and development.

The Peruvian Aprista party, or Apra, founded and led to this day by Victor Raul Haya de la Torre, has been a major power contender in Peru and inspiration for the generation of moderate reform parties that sprang up throughout Latin America in the 1930s and 1940s. (In fact they are sometimes referred to as the Aprista parties.) Explanations for the rise and political survival of Apra have generally focused on the party's dynamic, charismatic leader. While Klaren recognizes Haya's considerable talents and magnetic personality, he searches deeper for reasons behind Peruvian susceptibility to Apra's reformist appeal.

The party arose and has always found its primary base of support on Peru's north coast. When we look at the north we find that it differs economically from the rest of the country Since the latter part of the last century, it has been under the domination of large, highly efficient plantations that produce cane sugar for export. Klaren traces the origins of Apra to this geoeconomic situation. "Haya, along with other fellow northerners, became the political spokesman for various segments of the population in that area who in one way or another were dislocated and frustrated by the rapid break-up of the traditional society" (p. xv). Generally speaking, the process of modernization during this period was confined to the northern sugar area, but in the other parts of Peru where it also occurred, Apra likewise built a base of support according to the book.

To test this geoeconomic hypothesis, Klaren carefully reconstructs the socioeconomic and political histories of two important sugar-producing valleys near the city of Trujillo. With an impressive array of data, the study documents the fascinating process which ultimately thrust the Aprista party on to the national scene. Beginning in the 1870s outside interests supported by foreign capital started to buy up land for the cultivation of sugar. By 1927 there were three huge, modern plantations where once there had been 60 traditional haciendas. This modernization dislocated several groups that eventually formed the core of Apra. First, the labor requirements of the sugar plantations created a rural proletariat composed of Indians from the highlands who quickly fell victim to a vicious debt-peonage system. Labor unrest became commonplace in the valleys early in this century. Second, the modernization of sugar production wiped out small farmers and sharecroppers, forcing them to work on the plantations. Third, the sugar boom, which first stimulated local commerce, subsequently led to its demise as the large estates vertically integrated. Finally, the local intelligentsia, led by Haya de la Torre—whom the author pictures as something of a clever opportunist—joined the cause.

In 1931 Haya returned from exile in Europe (the exile period is not particularly well integrated into the author's analysis) to assume leadership of the diverse coalition of disaffected northerners and to contest a hastily scheduled presidential election. The Apristas sought to carry their message, radical for its day, to all Peru, but their support, which came primarily from the north, was not enough to win the presidency for Haya. Frustrated by electoral failure, the Apristas turned to violence which brought them into conflict with the armed forces in the Trujillo revolt of 1932. This confrontation poisoned relations between the Apristas and the military, although in 1968 the armed forces would appear to have adopted the party's appeal to those dislocated by modernization without having accepted Apra itself.

Peter Klaren's study of the origins of the Aprista party joins a growing body of literature on Peru that focuses on the impact of economic change at the local level. The author provides only a partial test of his geoeconomic thesis since he examined only one area in Peru. He does, however, mention similar developments in other Latin American countries in the conclusion. In fact the book cries out for cross-sectional, cross-national, and longitudinal analysis. This observation should not be taken as a criticism, but on the contrary, as an enthusiastic response to Klaren's effort to refine our understanding of modernization and development.

TERRY L. MCCOY

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**Legislatures in Comparative Perspective.** Edited by Allan Kornberg. (New York: David McKay Co., Inc., 1973. Pp. 457. $6.95, paper.)

**Comparative Legislative Behavior: Frontiers of Research.** Edited by Samuel C. Patterson and John C. Wahlke. (New York: John Wiley & Sons, Inc., 1972. Pp. 311. $14.95.)

A thorough reading of the two volumes under review should make it quite clear to even the most sympathetic reviewer that the comparative study of legislative behavior and institutions is still in its prehistory, still a collection of interesting but unrelated research efforts in desperate need of an integrative theory. Veteran observers of the comparative art will note more than a touch of irony here. The movement of recent years away from whole-system comparative studies was fueled by a widespread consensus that theory was inadequately connected to empirical research, and that smaller-scale efforts would be more effective in eventually building up a theoretical and empirical base upon which grand theory could rest. It now seems apparent that the same problems have been transported to the subfield of comparative legislative behavior. In fact, the disparity between theory and empirical research seems more glaring than ever, because the concentration on specific institutions has facilitated narrow-gauge empirical studies without corresponding theoretical integration, to the point where the subfield seems in danger of splitting into a series of related sub-subfields. The twenty-seven essays in these two volumes could be scaled along three axes: *scope* (from internal structure of a single legislature to external interaction of several legislatures), *method* (from qualitative descriptive to quantitative data-oriented), and *area* (from developed, highly institutionalized to underdeveloped, weakly institutionalized). The interests of the authors range from the quaint practices of the nascent "legislatures" of the South Pacific to the career patterns of past justices of the Iowa Supreme Court, but the reader is left with the impossible task of assembling the pieces of this puzzle into some coherent pattern. I think many readers will feel (as I did) both admiration for some of the individual contributions and distress at the overall lack of an integrating conceptual analysis which would have given these efforts added meaning. Although a respectable number of the articles are interesting and perhaps even stimulating in themselves, they have simply no overall cumulative effect.

While the inherent and as yet insurmountable difficulties of the subject are responsible for much of this lack of focus, part of the problem seems to lie in the editors' failure to provide an integrating theme around which the contributions could be organized. Allan Kornberg attempted to organize his volume around the concept of "institutionalization" of legislatures (the theme of a conference from which these papers were drawn), but he ruefully notes that "only four of the [seventeen] papers can be said to focus explicitly on institutionalization although another half-dozen do give more than passing consideration to the concept" (p. 1). Undoubtedly Professor Kornberg faced insuperable difficulties in trying to force his individualistic colleagues to lie down together on the procrustean bed of "institutionalization," but the result of this failure (for which no one who has ever tried to organize a panel could blame him!) is that the papers in his collection, by his own admission, "have not been given sufficient conceptual unity by a common focus" (p. 16). They represent dots on the widest possible landscape.

Perhaps most distressing, the more broadly analytical pieces seem to me the least successful contributions in the volume. Richard Sisson's "theoretical exploration" of legislative institutionalization is largely devoted to substituting idiosyncratic jargon for common used (and generally clearer) terminology. Is it of any possible help to anyone interested in legislative autonomy to dream up a phrase like "institutional/generic means of environmental control" to mean nothing more than the constitutional rights, either traditional or legal, of the "relevant unit" to manage its affairs and to control the allocation of values? Undoubtedly, further clarification and definition of terms is vitally necessary to this field—for example, the very term "legislature" seems inappropriate to many of the institutions discussed in this collection—but Sisson does not provide any conceptual clarification or operationally useful propositions through his manipulation of words. Similarly, Fred Riggs's loosely organized paper, aptly titled "Some Thoughts on Elected National Assemblies" becomes largely an exercise in word-play and ratiocination. For example, from the simple notion that legislatures can be described according to the degree of cohesion of their parties, Riggs first defines the two extremes of "disciplined" and "fluid" assemblies, then adds an intermediate category of "restrained" assemblies, and then follows this with an absurd ninefold permutation of these categories (i.e., fluid, fluid-fluid-restrained, fluid-restrained, etc.). Furthermore, Riggs's attempt to use precise language (to "avoid using neologisms") leads to some incredibly complicated statements that conceal rather commonplace thoughts, such as: "The balanced elected assemblies to be found in assembly-oriented (parliamentary) constitutive systems in open balanced polities are likely to be more disciplined than those found in not-assembly-oriented (presidential) constitutive systems in open balanced polities" (p. 68). I remain unconvinced that this is a genuine advance in theoretical clarity or precision. The logically distinct categories which Riggs devises seem virtually impossible to apply to empirical research, and he devotes too much time to inventing new nomenclature (such as, for example, "monotonic" and "isotonic" systems) which can be more of a hindrance than a help to conceptualization of the subject.

A good and healthy contrast to these forays into "pure" theory is the modestly conceived paper by Peter Gerlich, in which he gathers and analyzes historical data in order to infer some interesting tentative explanations for the differing rates of institutionalization of European parliaments. Of the remaining broadly comparative and conceptual pieces in the volume, the discussion by Malcolm E. Jewell ("Linkages Between Legislative Parties and External Parties") is informed and judicious, but fails to organize the materials and observations into some reasonably systematic conclusions. The most satisfactory analytical paper in this collection is provided by Andrew J. Milnor and Mark N. Franklin, who provide an interesting taxonomy of forms and contexts of opposition, enlarging the old and limited constitutional definitions of oppositional activity into the wider arena of modern politics, and relating oppositional activity to the type of issue involved.

The most impressive and stimulating articles in the volume, however, are those that build (or test) theoretical propositions on a solid data base. In this category, the essay by Samuel C. Patterson, John C. Wahlke, and G. Robert Boynton represents a most admirable achievement. Starting from an exceptionally clear and thoughtful discussion of the various functions of representative bodies, the authors decide to examine the function of generating "diffuse" and "specific" support for legislative institutions. They utilize data from a survey of the public, party leaders, lobbyists, and legislators in Iowa and expand this with less complete data from thirteen other American states, and finally with some sketchy but suggestive survey data from Britain, Germany, and France. The result is a highly successful blending of theory and empirical methodology. Also of interest in this category are the preliminary report by Richard I. Hofferbert and Ira Sharkansky of an ambitious project to relate socio-economic factors and political attributes of subnational political units at very different stages of modernization; and the paper by Allan Kornberg et al. concerning parliamentary recruitment in Canada.

Several quantitative papers in the collection neatly demonstrate conceptual pitfalls in the organization of the research design and thus essentially fail to show relationships at the usually acceptable levels of significance. John G. Grumm's attempt to operationalize Easton's input-output model at the simplest level is innovative and potentially useful, but the simplifying assumptions result in the use of indicators that are not sufficiently refined to tap the complexity of the input structure of these systems. The article by William Mishler et al. attempts to determine the extent of freshman cue-taking from older members of the House of Representatives through multiple regression of voting by the two groups—a weak, indirect, and ultimately inconclusive (but undoubtedly more convenient) method when compared with the alternative of collecting interview data. It should be pointed out that stronger correlations in this case would not have demonstrated that cue-taking is the correct causal inference. Another innovative paper by Chong Lim Kim on "Consensus on Legislative Roles Among Japanese Prefectural Assemblymen" points the way toward possibly productive future research but uses indicators that are far too crude to elucidate the questions that the author asks.

The volume edited by Samuel C. Patterson and John C. Wahlke is apparently intended to represent the "frontiers of research," and perhaps for that reason the authors are somewhat more self-conscious about methodological questions. Gerhard Loewenberg provides a very thoughtful and readable critical overview of the field. Unfortunately, the defects that he notes in his essay are demonstrated in the remainder of the book. Professor Loewenberg points to the "inadequacy of theory to guide legislative behavior research" (p. 16), but the book does not contain a single attempt to develop such theory; he warns that the choice of problem has too often been determined by the "desire to employ particular analytical techniques rather than to solve problems of theoretical relevance" (p. 19), a phenomenon which seems to me well demonstrated in the article by Hans Daalder and Jerrold G. Rusk on "Perceptions of Party in the Dutch Parliament." This article, especially in the beginning portions, does provide some interesting information and observations on the perceptions of Dutch parliamentarians, but as the article progresses, the authors become increasingly preoccupied with measurement techniques, devoting the last half of their paper to a prolonged discussion of the various attributes of the multidimensional scaling techniques developed by Kruskal and Coombes. While the authors' adaptation of the data to their statistical methods seems to me highly questionable, the main point is that the primary focus of the study has by the end shifted from comparative legislative behavior to comparative statistical methods.

Indeed, the entire notion of comparative research is weakly represented in this volume. Of the eight research papers (excluding overviews) in the collection, only one is cross-national in scope, and even that one (by Mogens N. Pederson, "Lawyers in Politics: The Danish Folketing and United States Legislatures") is unbalanced and unsystematic in presenting comparisons. What we have in this volume is a set of interesting, even exceptionally well-done studies of individual national legislatures or sets of subnational legislatures, asking different questions, using different methods, and finding different answers. A search for common themes or plausible interconnections among these studies proves almost totally frustrating. For example, the article by Heinz Eulau, based on a survey of 82 city councils in the San Francisco area is a superb piece of research and analysis. But what are the conclusions that one can legitimately draw from this study? Eulau convincingly demonstrates certain patterns of behavior and their relation to structural and situational factors, but have we learned something merely about San Francisco city councilmen, about small (mostly five-member) "legislative bodies," or about interaction in small decision-making groups in general? Is Eulau's analysis relevant to the study of large national legislatures? Of course, Professor Eulau can not be held responsible for this difficulty; but the point remains, in the words of the editors, that "in the United States experience, we simply have not dealt with the question of the idiosyncrasy of legislative institutions operating for different territorial units" (p. 293). Similarly, the study by John A. Brand of the attitudes of voters and councillors in three Scottish cities is an exceptionally well-conceived piece of survey research and analysis, but it may very well be irrelevant to other localities, especially in view of the author's conclusion that "each city appears to have its own cultural and institutional patterns that impose themselves on politicians and laymen in all parties . . . [and] which seem to stamp the relationships in every issue with a distinctive local character" (p. 265). Of the remaining articles in this volume, the one by Peter Gerlich on attitudes of Vienna city councilmen is informative and provides some rough comparisons with earlier American findings; the study by John E. Schwarz and Geoffrey Lambert of the voting behavior of British Conservative backbenchers attempts, through a questionable set of assumptions, to relate voting behavior to the backbenchers' aspirations for higher appointment and to the marginality of their constituencies; and John G. Grumm's essay is an attempt to test the notion of feedback by using aggregate data (over a 15-year timespan) from the American state level—a serious conceptual mistake, since feedback, at least in Deutsch's and Easton's conceptions, was the resultant of specific outputs and could not be measured by the overall amount of governmental activity (in welfare expenditures or any other gross measure). Also included in the volume is a description of the parties in the Finnish parliament by Pertti Pesonen, and a survey of recent research by the editors.

JEROME M. GILISON
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**Dianying, Electric Shadows: An Account of Films and the Film Audience in China.** By Jay Leyda. (Cambridge, Mass.: The MIT Press, 1972. p. 515. $12.50.)

In the midst of a spate of recent books and articles on China produced by "post-ping pong thirty-day wonders," one welcomes a book by one of the few Americans to have had the opportunity to live in China for an extended period during the years of Sino-American hostility. Jay Leyda lived in Peking from 1959 to 1964 and worked in the Film Archive of China (established in 1957), primarily helping to catalogue their collection of foreign films, but also providing his hosts with comments and advice on the many Chinese films, new and old, he had the opportunity to see over this period. These viewings of Chinese films, supplemented by Mr. Leyda's consultation of film archives in several other countries, form the primary basis of this book.

As a history this book provides the most comprehensive treatment available in English of the development of the Chinese film industry from the early years of the 20th century to 1967. Mr. Leyda's personal contacts and years in Peking also provide a fair amount of new biographical detail about the lives of Chinese film-makers and artists, and their struggles to produce effective films in the midst of turbulent political upheavals and pressures. Finally, the author offers useful critical commentary on films covering this entire two thirds of a century.

The result is often valuable as a general sourcebook on Chinese films, but a number of shortcomings lessen its usefulness as either a critical study or a treatment of how artistic and political institutions interact. Mr. Leyda has crammed his book full of all kinds of information, much of it scarcely related to his central themes. This is particularly annoying in the chapters dealing with the years he worked in China, onto which he has tacked personal memoirs, including diary notes, poems he scribbled to himself, and incomprehensible jottings about people he knew but we don't. Furthermore, his goal of producing a comprehensive history of Chinese films often gets in the way of his critical analysis. In many places we are given listings or short plot summaries of films which, unfortunately, Mr. Leyda didn't get a chance to see. Thus the reader interested in his critical evaluations of Chinese films has to wade through a good deal of chaff to find the films which are treated in enough detail to be very meaningful.

While my primary complaint is that this book is cluttered with too many divergent types of information—and here the lack of a forceful editorial hand must take much of the blame—there are some curious gaps. The book's subtitle tells us it is partly about the film audience in China, but in fact very little is said on this topic in the text (understandably, since Mr. Leyda saw most of his films in private screenings within the Film Archive of China offices). Also, Mr. Leyda has written a number of books about Soviet films and speaks Russian (while he doesn't speak Chinese, and apparently relied on a constant interpreter to make sense of the Chinese films he was viewing), but when he tries to deal with the conflict between artistic expression and Party controls in China he makes almost no reference to parallel or divergent Soviet experience in this area, which would have formed a useful comparison.

Despite these defects, Mr. Leyda's critical conclusions are worth summarizing. He feels on balance that the most creative and dynamic period in Chinese films was in the 1930s and 1940s, when film-makers struggled against Kuomintang and Japanese oppression to make films of social value and human meaning. While fine films are not lacking in the post-1949 period, the transformation of film-makers to employees of the official propaganda organs has often resulted in the stultification and abuse of some of the finest talents in Chinese films, a process affecting other arts as well. This process reached a climax in the Cultural Revolution (1966–69), when many of the artists Mr. Leyda knew were attacked and their pictures withdrawn. As Chinese film studios have reopened in the last few years they have been more sharply bound by orthodox film formulas than before, and one can only wait and hope with Mr. Leyda that the creativity he reports for earlier periods will some day emerge again.

MARTIN KING WHYTE
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**Honecker and the New Politics of Europe.** By Heinz Lippmann. Translated from the German by Helen Sebba. (New York: The Macmillan Company, 1972. Pp. 272. $7.95.)

Erich Honecker succeeded Walter Ulbricht in May 1971 as First Secretary of the East German Socialist Unity Party (SED) in a relatively smooth transfer of power. This biography, which covers Honecker's career until he became party chief, is a welcome addition to the limited English-language literature on the political leadership of the German Democratic Republic (GDR). Although not conceptually comparable, it provides a companion piece to the recent studies of Peter C. Ludz.

Heinz Lippmann's credentials are interesting because of his long personal acquaintance with his subject. Presently living as a journalist in West Germany, Lippmann was for many years an official of the SED, and from 1946 to 1953 he served as Honecker's deputy. Detailed first-hand accounts of Honecker's style of leadership, his personality and character, and his close relationship with Ulbricht make the book valuable to observers of contemporary European affairs even though the author makes no claim to offer a definitive work. The background and accomplishments of the future SED leader are traced chronologically, and little effort is made to place the subject in his historical or political context other than in the East German one. Thus as a biography the book is somewhat narrow, its focus on Honecker depriving the reader of a sense of the great dramatic developments of postwar Europe surrounding the GDR.

Erich Honecker's childhood, we are told, was characterized economically by material deprivation and politically by the militancy of his family environment. At the age of six, he entered politics, working for his father as a distributor of leaflets demanding better conditions for the mineworkers of the Saar. At ten, he joined the Communist youth organization, the Young Pioneers. He joined the German Communist Party at seventeen in 1929, spent the next years in anti-Nazi political activities, and in 1935 was interned for what became a decade of imprisonment. After the war, Lippmann tells us, Honecker ran the Free German Youth (FDJ) organization, and during this period developed what Lippmann describes as keen tactical skill as a Party infighter. Honecker's ambitions coincided with his ideological tenets, and he became a close collaborator of Ulbricht, whose guiding principle was that East Germany and the Soviet Union should remain closely allied. On occasions when Honecker's judgment faltered, he was rescued by Ulbricht—incurring debts which he later repaid at times of intra-SED challenges to Ulbricht's leadership by his strict loyalty. Like his patron, Honecker was always aware of the indispensability of Moscow's support. Perhaps more than Ulbricht, he felt a deep emotion toward the Soviet Union; on the occasion of Stalin's death, says Lippmann, "his voice faltered, and tears ran down his cheeks" (p. 152).

Honecker emerged as heir-apparent in 1958, when with Ulbricht's support he became a full member of the Politburo, in charge of armed forces with responsibility for the security of the GDR. In 1961, in Berlin, he functioned as chief supervisor and builder of the Anti-Fascist Protective Wall, an accomplishment for which Lippmann almost praises his former colleague. The ultimate responsibility lay with Ulbricht and Khrushchev, the author notes,

> But it was Erich Honecker who actually built the Wall. It was he who took charge of all the organizational and military preliminaries, supervised security while it was being built, and planned and directed the political and propaganda campaigns connected with it. The coordination of the vast military, organizational, and political plans that culminated in the utterly unexpected erection of the first barricades was entirely in his hands (p. 186).

This operation strengthened Honecker's position in the Party even further, and during the 1960s he continued to take hard-line stands on both domestic and international questions. When Warsaw Pact forces occupied Czechoslovakia in 1968, he welcomed the use of military measures to counter antisocialist developments. As *Ostpolitik* began to unfold in Bonn, Honecker was skeptical of Brandt and hesitant to destroy the carefully shaped image of the Federal Republic as an enemy. The latter is one of the few known examples of Honecker's disagreement with the preferences of the Soviet leadership.

Lippmann's picture of the SED chief is one of a conservative figure, pragmatic and thorough, neither mechanical nor flamboyant. When Honecker initially assumed power, reactions in the Federal Republic were pessimistic. The *Frankfurter Allgemeine Zeitung*, a leading West German newspaper, predicted editorially that "the moment will soon come when Ulbricht will be remembered with a kind of dialectic regret" (May 4, 1971). Since then, however, there has been a steady, although uneven, trend away from earlier cold war rigidities. But here Lippmann's account ends, and we must search elsewhere for the record of Honecker's performance as party leader and for the broader international context within which he operates. Much of the information which would have been necessary for a more complete account is simply unavailable, of course, but Lippmann might have enriched his sketch of Honecker by the addition of social, political and economic material.

Prospective readers should hence be advised that the book's title is somewhat misleading in the English-language edition: this is *not* a book about the new politics of Europe. The original German title, *Honecker: Porträt eines Nachfolgers* (Honecker: Portrait of a Successor), should have been retained. Helen Sebba's translation is quite satisfactory, and there is no sense of awkwardness in the text.

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**The French Budgetary Process.** By Guy Lord. (Berkeley: The University of California Press, 1973. Pp. 217. $12.50.)

Guy Lord's short book is the first full-length study of non-American budgeting from a political (largely Wildavskian) perspective to see print. His questions are: "What are the rules which govern the process? Who are the main actors? What are their functions? When and how do they intervene in budgetary decisions? What are their goals, and how do they seek to achieve them? In short, how are decisions made when the budget is prepared, discussed, and voted?" (p. 188). The book is based on some 60 interviews with participants, along with newspaper accounts and secondary works (mainly on public finance and bureaucracy).

The first three chapters are straightforward accounts of "The Development of Financial Procedure" from the Revolution to the late 1960s, "The Constitutional and Legal Framework," and "Stages and Actors" in the process. Budgeting was of course greatly affected by the Gaullist reforms of 1959, which reduced Parlement to a marginal role and gave the President a significant coordinating and even initiating function. More subtly, the influence of the Ministry of Finance within the administrative process increased as economic aspects of budgeting were given greater weight, and as growing complexity produced tighter deadlines and a tendency to base decisions on administrative or financial criteria rather than political considerations. As Lord points out, the details of organization and procedure outlined here in some detail not only provide the framework for budgetary politics, but themselves become important strategic resources for the participants who understand them best.

The heart of the book is Chapter 4, "Attitudes and Strategies." Few students of budgeting will be surprised to learn that the French process is highly incremental, or that spending ministries emphasize the needs of their own clienteles, while Ministry of Finance officials see themselves as rising above special interests to decide unselfishly and objectively what is best for the nation. Still, it is startling that the repertory of budgeting behaviors appears to be so narrow: Lord's list of participants' strategies closely resembles those in independent accounts of other budget systems. (Very probably budgetary calculations are similarly universal, although Lord does not treat this aspect in detail). On the other hand, Frenchmen are not Americans, or Japanese. The budget subsystem lies too close to "the authoritative allocation of values for a society" not to reflect the style of the broader political system. The most interesting peculiarities in the French case are the historic and continuing tension between government and legislature, which continues to dominate much writing about budgeting; the tendency to meet political problems with legalistic responses; and the pattern of extreme fragmentation and institutional autonomy called *cloisonnement*. Professor Lord, following Michel Crozier, particularly stresses this last factor as producing many conflicts—the field vs. Paris or division vs. division within each ministry, spending ministry vs. Ministry of Finance—which cannot be resolved through hierarchical administrative channels. Rather unfriendly horizontal negotiations are the norm, and they often end in arbitration at political levels. Many overlapping structures, liaison conferences, and other bridging devices seem to be needed for the system to work.

Lord deals with his intended subject so competently the reader might wish for a touch of speculation in a broader context. Even though the data available at time of writing were probably insufficient for specific cross-national comparisons, a more comparative perspective might have allowed preliminary guesses about a few general questions. For example, what proportion of total domestic policy output is handled through the budgetary system in France? How incremental, relatively speaking, is French budgeting? What is the balance between top-down, policy-oriented "macro" decision making and bottom-up "micro" decision making? Somewhat similarly, even if rigorous quantitative analysis is unfeasible, one still might try to ask "what difference does it make?" from time to time—do variables of process actually influence budgetary outputs? For example, does the loss of parliamentary influence mean that a smaller share of governmental resources will go to vote-based interest groups, like veterans? Does increasing complexity and more Ministry of Finance power mean more or less fluctuation in budget shares? How can variations in policy expenditures cross-nationally or over time be explained? Books such as *The French Budgetary Process,* with its detailed description and sensitive analysis of relationships among budgetary actors in one interesting country, provide the foundation for asking questions like these in the future, as well as expanding our understanding of contemporary governmental politics.

JOHN CREIGHTON CAMPBELL
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**Buganda in Modern History.** By D. A. Low. (Berkeley: University of California Press, 1971. Pp. 277. $8.75.)

The erstwhile kingdom of Buganda has long fascinated social scientists. Until its destruction in 1966, Buganda represented a unique amalgam of apparently contradictory elements. All observers noted, on the one hand, the pomp, hierarchy, symbolism, and power of the Kabaka (the monarch). On the other hand, however, they discovered the adaptability, dynamism, and political awareness of the Baganda. (Following Bantu spelling, in which prefixes are used extensively, let the reader be alerted that *Bu*ganda refers to the kingdom, *Ba*ganda to the people, with *Lu*ganda the language spoken by an individual *Mu*ganda.)

Professor Low makes clear that this scholarly fascination is amply justified. *Buganda in Modern History* is a set of essays—some brilliant—that collectively and episodically portray the kingdom from precolonial times to the 1966 sacking of the Kabaka's palace. He describes two "revolutions"—oligarchical and Christian—that accompanied and amplified the external pressures on the state in the late 19th century. These "revolutions" were linked to a distinctive tension in Baganda society between two norms of social organization. Each Muganda belonged to a clan (*bataka*); the clans were egalitarian, paradoxically, through their ascriptive bases. At the same time, a strong hierarchical system existed, its significance enhanced by the oligarchical "revolution." Each Muganda was linked to a chief in a patron-client relationship (the *bakungu* nexus, based thus on subordinate-superior ties). Atop both the clan structure and the pyramid of commoners and chiefs sat the Kabaka. He united the two means of social organization in Buganda, each of which, in yin-yang complementarity, permitted adaptation under diverse circumstances.

None of what I have noted thus far will surprise those familiar with David Apter's *The Political Kingdom in Uganda* (1961). Yet Apter, in his concern for "bureaucratic nationalism," concentrated more on the oligarchical transformation than on the populist awakening. Apter tended thus to focus more on the *bakungu* pattern than the *bataka* pattern, as suggested in the previous paragraphs. In one of the curious ironies of history with which Buganda abounds, the populism that arose did not take the form common elsewhere in Africa, that is to say, the dominant, mass-based political party. Political movements that were founded by young, Western-educated Baganda foundered on the skepticism of farmers, who saw these upstarts as likely to become "only one more generation of dominating chiefs" (p. 154). Populism followed the *bataka* pattern (egalitarian), and became focused on the person of the Kabaka, in his guise of head of all clans.

How did the British view this bundle of enigmas? Low examines the parentheses of the colonial period, fittingly the symbolic beginning and end of imperial rule in East Africa. Buganda helped significantly change British public opinion in 1892. Numerous petitions from diocesan conferences, chambers of commerce, and public meetings, Low suggests, "heralded the beginning of the end of that mid-Victorian anti-imperialism which had on most occasions fought against the creation of 'formal' empire" (p. 82). The parentheses of colonial rule started to close in 1954, when a constitutional conference was held in which "the notion of a primary African state [in East Africa] had for the first time been given constitutional specificity" (p. 130). With self-government assured, little urgency impelled nationalistic parties to press their claims, accounting for the otherwise surprisingly low level of political commotion (apart from the temporary exiling of the Kabaka) in the 1950s.

*Buganda in Modern History* offers only partial glimpses into the rest of Uganda: for example, into the replication of the chief-subordinate relationship outside Buganda, into the slow awakening of political movements, or into the dominance of the Baganda in the civil service. Elsewhere in Uganda, envy grew against the riches, exclusivity, and adaptability of the Baganda, leading toward the 1966 bloody battle between supporters of the Kabaka and troops of the central government. Despite the formal abolition of the kingdom and the 1971 installation of the idiosyncratic Amin government (effected after completion of the manuscript), the energy and educational accomplishments of the Baganda continue to make them indispensable to Uganda's functioning.

Low's thematic book does not resemble the ordinary study of African politics or history. *Buganda in Modern History* is not a smooth, chronological recounting, nor is it focused upon an *idée fixe* to the exclusion of other arguments. Each chapter is self-contained, five of the seven chapters having been published earlier as articles. Overlap and disjunctures remain, as the author recognizes. Should the book be purchased? Yes, I suggest, for *Buganda in Modern History* is a useful, more readable counterpart to, and extension of Apter's 1961 book, and is a coherent compilation and re-editing of already published significant material. Few African societies have enjoyed such substantial analyses—yet perhaps no African society has been as important to social scientists as the Baganda.

CLAUDE E. WELCH, JR.
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**Daily Life in Revolutionary China.** By Maria Antonietta Macciocchi. (New York: Monthly Review Press, 1972. Pp. 506. $15.00.)

One imagines the tour guide, his Mad-Hatter headpiece advertising destinations throughout People's China, standing near Tien An Men in Peking, hustling tourists into his bus. And then we're off, on a tour ever less magical and mysterious, with an itinerary at least as formal as the one that takes the provincials from Times Square to Greenwich Village. First it's off to the summer palace, where the splendor of feudalism has been proletarianized as a reminder of the oppression that was its obverse, now to the university with its scholars chastened by their remolding and speaking humbly, on to the transistor factory set up and run by proud former housewives, now to the school where deaf-mutes are treated successfully by dedicated soldiers using acupuncture techniques they mastered through dangerous experiments on themselves, next on to the May 7 School for the Peking district where contrite cadres have been taught to respect common people by laboring among them. And so it goes, a series of set-pieces, Macciocchi's little different from those of dozens of other China-trippers, beneficiaries of improving relations betwen China and the West since 1970.

And yet for all their familiarity, many of these vignettes remain both poignant and interesting. It is through the presentation of these scenes that the Chinese portray themselves as they wish to be seen, as a people who have "stood up," in Mao's memorable metaphor, and as a nation-state of immense proportions whose members nevertheless feel and act out daily a sense of mutual involvement and collective purpose. "Confronted by such a collective presence," Macciocchi quotes another writer as observing, "Westerners (except peasants, the poor, and some rare exceptions) tend to feel awkward, stingy, and 'slobbish' . . ." (p. 107).

This is no less true of Macciocchi herself, a Communist member of the Italian Parliament, journalist-editor, and specialist on Asia and Africa, who was made reluctant to complain about a sudden and excruciating pain in her ear by the accounts of revolutionary self-sacrifice and courage she was listening to at the time of its onset. Perhaps this accounts for a kind of studied ingenuousness that appears from time to time in her comments. [The Chinese "don't ever seem to get mixed up when they talk. . . . They all seem to be born speakers and lecturers," the author says (p. 62), as if they had not said the same things in the same way to other foreign visitors and to each other in political sessions many times before. As a result of the Great Leap Forward, she observes farther on, "the peasants became adapted to the industrial process and established a closer relationship with the working class" (p. 122).] And too much of the book consists of verbatim transcriptions of stilted talks by her hosts, without sufficient analysis to help the texts overcome a triteness that is not mitigated by the sincerity with which they are delivered.

Nevertheless the book as a whole transcends both the limits of the experiences the author was permitted and the familiarity of the form (she was in China in late 1970, well before the numerous Americans whose writings, made available more quickly in the United States, account for this familiarity), on the one hand, and the Westerner's tendency to make of China what he needs, on the other.

There are a number of reasons for this, not the least of which is that the immensity of change and the new possibility of human dignity in China cannot be trivialized to the level of a tourist attraction, no matter how pat the presentation. The author sees these somewhat abstract experiences of "liberation" as essential to the release of energy we associate with development. When she does mercifully abandon (p. 135) her injunction to herself to "Be modest," and simply transmit without interpretation an account of her experience, the author describes China as the enormous experiment that it is, the laboratory for an attempt to carry out a unique and human-centered program for development. Particularly in her chapters on industrialization, medicine, and women in China, Macciocchi portrays effectively the interplay of efforts to expand China's resources and the determination to ensure egalitarianism in their distribution. She seems to appreciate the Chinese ambivalence to technology, which is essential to great achievement, for example, in space and weaponry, and at the same time has the potential to become "a new kind of barbarism," (p. 129), creating hierarchy and contributing to a politics the Chinese reject. And as a Marxist, the author appreciates the vital role the Chinese assign to ideology, a subject not always given its due in Western books on China, as a bulwark against the revival of the rejected tendencies. This understanding is woven into her interesting discussions of schools, cultural life, the use of Mao's writings, and the pervasive continuing consciousness she found throughout China of the critical significance of the Cultural Revolution.

Nevertheless, with all the author's advantages in her quest, there remains a feeling that something is missing, as with virtually all accounts of foreigners visiting China. Are we wrong to wonder where is the ripple of anger disturbing the placid surface of collective life, where the cadre who resents being "remolded," the peasant who is offended by the pervasiveness of politics? Can we do anything more than admit that in this sense the Chinese will remain (one winces to say it) inscrutable?

PHILIP E. GINSBURG
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**The Bolshevik Seizure of Power.** By S. P. Melgunov. Edited by Sergei G. Pushkarev. (Santa Barbara, Calif.: ABC-Clio Press, 1972. Pp. 260. $15.00, cloth; $5.50, paper.)

This is a superb book, published originally in Russian in Paris in 1953, and now available in good English translation by James Beaver with excellent editing, bibliography and indexes. The abridgment is limited and carefully done to preserve essentials. The original Russian title, "How the Bolsheviks Seized Power," describes the precise theme—the circumstances and the actions which enabled the Bolsheviks to seize and maintain power in October/November 1917. The author, S. P. Melgunov, was a distinguished historian, and an important political leader of the revolutionary era—the head of the People's Socialist Party.

Melgunov emphatically rejected a determinist interpretation of the Bolsheviks' victory; " 'October' was *not the realization of* 'February.' Only mistakes of those who were able to prevent the seizure of power by the Bolsheviks made it 'inevitable' " (p. 3). He shared Kerensky's view of the importance of the Kornilov Affair in undermining the provisional government and strengthening Bolshevism, but credited the style of Kerensky's response to Kornilov with giving much unnecessary support to the Bolshevik cause. His book illustrates clearly the lack of defensive preparation by the moderate socialists as the Bolsheviks prepared their coup, and the strange role of Kerensky in preventing the sending of troops to Petrograd to regain control from the Bolsheviks once the coup had occurred. Moreover, Kerensky's government was so severely discredited as to render vain all hope of rallying anti-Bolshevik opposition around a scheme for its restoration.

Melgunov evidently believed that only the leaders of "revolutionary democracy"—the non-Bolshevik socialists—could have put an early end to Bolshevism. Their failure to do so is explained by a combination of their own errors of judgment and the aggressiveness and determination of Lenin. The chief error was a tacit assumption, widely shared (even by many Bolsheviks), that Bolshevik power would not last, an assumption buttressed by reluctance to initiate armed action against a fraternal socialist group. Hence the vain pursuit of a coalition government in the first days after the Bolshevik coup, and, once that hope was shattered, the placing of all faith in the Constituent Assembly. The strong showing of the Bolsheviks in the mid-November Constituent Assembly elections merely strengthened the reluctance to act against them, and the non-Bolshevik socialists drifted helplessly to the denouement in January 1918 when the Bolsheviks forcibly dispersed the Constituent Assembly.

There are many remarkable features of the book, not least of them the presentation of

Lenin, whose ruthlessness and revolutionary fanaticism, and whose occasionally irrational behavior, are unusually well depicted—often in the words of fellow Bolsheviks. Other interesting points are the degree of spontaneous popular opposition to the Bolshevik coup, and the amount of simple thuggery used by the Bolsheviks against socialist opponents, as in vandalizing the offices and equipment of the opposition socialist press in the early days following the coup. Perhaps the most striking point made about Lenin is his utter lack of inhibition about the use of verbal and physical violence, and all manner of deceit, against opposition within and outside the party, to establish and maintain his monopoly of political power and his own conception of the revolution. It is scarcely surprising that he won against an opposition which "insisted on liquidating the October revolt without repression and was only willing to allow 'noncooperation' as a means of obtaining this objective" (p. 189).

Melgunov's presentation is eloquent testimony to his scholarly training and commitment. He is a reliable and perceptive guide to the extensive memoir literature on the events he describes, and is ever careful to indicate the limits of available factual information. He is a sharp critic of Lenin, but he is no less critical of Lenin's erstwhile colleagues. In his presentation, as in the events, the real heroes are largely obscure figures—such as the Bolshevik Lozovsky, a man of decency and principle who fought Lenin's exclusion from power of the non-Bolshevik socialists. But even this remarkable man clung to the premier political fallacy of Russian revolutionary democracy—that democracy was the concern and the right only of socialists. As Melgunov notes, whatever role the Constituent Assembly might have played in the struggle with the Bolsheviks, even the non-Bolshevik socialists had largely abandoned any commitment of principle to that institution. In an important sense, therefore, they were without an effective rallying principle as they faced Lenin; they were ideologically disarmed before the struggle began. The skillful development of this theme is perhaps the main contribution of the book.

HERBERT J. ELLISON
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**Twilight or Dawn: The Political Change in India.** By Iqbal Narain. (Agra: Shiva Lal Agarwala & Company, 1972. Pp. 244. Rs. 20.)

Professor Iqbal Narain perhaps ranks among the top five or so political scientists in India. In this book, much of which has already appeared as articles in various journals, he analyzes the impact of the fourth general elections of 1967 on the Indian political structure and examines the tensions and problems of transition from one-party dominance to multiparty competitive politics. Toward this end, he discusses, relying basically on newspaper materials, a series of issues bearing on state politics, center-state relations, coalition-formation, and certain key political roles.

His work demonstrates a remarkable ability to see the essence of a problem amid a heap of detail, to articulate it expressively and precisely, and to generalize from the immediate event or case to a wider universe, accompanied by a penchant for, and great skill at, categorization and classification. The discussion is always balanced and level-headed, bringing clarity and sanity to the issues of great debate in Indian politics. Although the book lacks thematic unity, tends to jump from one subject to another, and does not deal with any issue in depth, it is essential reading for the delineation of the institutional issues in Indian politics in the late 1960s.

The major theme of the book is that the fourth general elections constitute a political watershed, marking the end of one-party dominance and generating another phase which the author calls the politics of polarization. In the development of this theme, Narain adopts a measured and cautious stance but nonetheless one that is characterized by a robust though unsupported optimism and insistent approval. He urges that the new phase of the politics of polarization should be treated "as a sort of protracted middle range phenomenon on the continuum of Indian politics which opens with one-party dominance, and whose 'middle' may itself have to undergo several political transformations by way of its stages of growth towards the final state of political equilibrium at which the Indian polity may ultimately settle down as a democratic system" (pp. 73–74).

As a perspective this may be useful, but Narain constantly shifts the grounds for optimism and approval. If one-party dominance comes to an end, it is considered a desirable development for competitive politics; if competitive politics assumes unsavory forms, it may be considered disturbing but is necessary for the transition to a new political equilibrium; if the Congress party splits, it is seen as a necessary step toward polarization in Indian politics; and if one-party dominance is restored that too is hailed as a well-deserved end to the unhappy experience of competitive politics. It would seem that almost anything that happens within the Indian political system can be seen as either desirable or necessary for desirable ends.

The key weakness of the analysis, however, is its wholesale concentration on political epiphenomena. The focus of attention is entirely political structure, with no reference to the underlying economic forces and social classes. More important, there is little concern for the ends for which political structure exists or ought to exist—social justice, national integration and political independence. In the evaluation of political structure, Narain curiously apotheosizes differentiation and, flowing from it, the liberal-democratic framework, especially of the British variety, while he denigrates—through neglect—system performance in relation to development. Fundamental to this orientation, I believe, is the author's failure to appreciate the essential difference between the political features of *modernity* and the political requirements for *modernization*. This is unfortunate for Indian political studies, because they would benefit tremendously if Narain's great analytical skills and incisive intellect were also turned toward these issues of crucial concern.

BALDEV RAJ NAYAR

*McGill University, Canada*

**Area Handbook for Ceylon.** By Richard F. Nyrop, Beryl Lieff Benderly, Ann S. Cort, Newton B. Parker, James L. Perlmutter, Rinn-Sup Shinn, and Mary Shivanandan. (Washington, D.C.: Foreign Area Studies, 1971. Pp. 525. $3.50.)

The intent of this volume, one of a series of area handbooks prepared by Foreign Area Studies of American University, is to assemble and organize a large quantity of information on a wide range of subjects relating to modern Ceylon. In this, the volume seems successful. Although it contains little that is new to the social scientist familiar with the nation, it can provide a convenient starting point for the scholar (or other person) who is commencing study of Ceylon. The volume is divided into four major sections: "Social," "Political," "Economic," and "National Security." The longest, by a wide margin, is that titled "Social," occupying 224 of the 472 pages of text. The section on "National Security" is only 27 pages in length, but is probably as full an examination of the police, the military, internal security, and crime as exists. The emphasis is on description, giving to the narrative a rather flat and uninspiring quality. Nonetheless, the volume does, overall, seem to capture some of the complexities and incongruities of a nation involved in rapid social, political, and ideological change. That there are some factual errors, misinterpretations, and oversimplifications seems less surprising than that there are so few, given the wide range of information crammed between its covers.

A major limitation on the value of the volume results from the particular time it was written. The research and writing was completed in 1970. Although events stand still in no nation, the past four years have witnessed particularly breathtaking change in Ceylon. The adoption of a new constitution in 1972 rendered obsolete the description of governmental organization, and indeed outdated the title of the volume, for the nation is now officially the Republic of Sri Lanka. Other subsequent developments have had far-reaching political consequences. The statement that "students and youth in 1970 posed no immediate threat to the incumbent party or to the maintenance of public order" (p. 258) seemed a reasonable observation in 1970, but early the following year an armed insurrection of students and youths left at least 1,200 persons dead and some 14,000 in custody. The handbook, in short, can be useful as an introduction to Ceylonese society, but those using it should be alert to many changes, some profound, in the intervening years.

ROBERT N. KEARNEY

*Syracuse University*

**Toward Central Government Computer Policies.** By the Organization for Economic Co-Operation and Development. (Paris: Organization for Economic Co-Operation and Development, 1973. Pp. 215. $4.75.)

This monograph is the fifth in a series on government computer problems issued by the O.E.C.D.'s Committee for Science Policy. Prior volumes have focused on the implications of computer technology for public administration, communication, and the privacy problem. These issues are necessarily raised in this study as well, for it focuses upon governmental decision making about the structure, content, and utilization of public and large-scale private data bases. It surveys the state of the art and short-term plans of O.E.C.D. member-nations, and analyzes their common and divergent approaches to data-base development.

Data banks store a vast array of information—the budgets of governmental agencies, trade-flow statistics, and weather reports. This volume focuses upon the most nettlesome form of data, that organized around individual citizens or private organizations. Government file clerks at various levels long have squirreled away much of this information. The computer, a technological development, simply has encouraged them to convert their files to machine-compatible formats. It is an *administrative* development, the integration key, which makes it feasible to merge these files. Driver's license, land and vehicle ownership, tax liability, church membership, military service, arrest, and voting participation information can be merged practically into super files only when each citizen or business is assigned a unique identification number. This practice is currently most advanced in Germany, Sweden, and the Netherlands, the committee reports. In the United States, the social security number has enormous potential in this regard. Congress, however, has been vigilant in resisting the Office of Management and Budget's plan to establish a National Statistical Data Center along these lines. With the exception of England, where the Opposition (whichever party it happens to be) also opposes executive data-bank proposals, most O.E.C.D. governments have enjoyed less resistance to the development of administrative information systems.

With some simplification, these systems can be classified into two categories: statistical systems, which rely upon integration keys to associate data but produce only grouped or numerical output; and intelligence systems, which store and regurgitate dossiers on specific individuals. But without elaborate and costly safeguards the former can usually be turned into the latter, and both contain information that can be reused by persons and for purposes far removed from those ostensibly involved in its collection. In the United States, the left has been concerned about "overt" files on public-assistance recipients, those who collect unemployment insurance, credit applicants and arrestees, as well as "covert" data banks on black leaders, political activists, and those who have come to the attention of the F.B.I. and the I.R.S. The right has aimed its guns at meddling by the Bureau of Census, which they argue has gone far beyond its constitutional mandate to count heads. What both groups are calling for is what this book is about—policy-making concerning the collection, integration, use and reuse of data on citizens.

The report concludes that there are in practice three solutions to the problem. Some member governments monopolize the rights to these data bases and maintain their own systems at the highest level to process them (Finland, Denmark); some attempt to coordinate and contain data base construction and use at the ministerial level (France, Holland, Sweden); and several pursue anarchistic nonpolicies that the committee euphemistically dubbed "partial co-ordination" (the United States, Canada). The latter approach consists of "organizing interaction among ADP users rather than imposing decisions on how their systems should be developed or operated" (p. 39). While this does not mean that anything goes, a lot does, and it is in the partially coordinated countries that disputes over who has access to public and private files, to what uses they may be put, and what power people have to review the accuracy and relevance of their files' contents, have been most prominent.

WESLEY G. SKOGAN

*Northwestern University*

**International Population Census Bibliography: Europe.** By the Population Research Center, Department of Sociology, The University of Texas. (Austin: Bureau of Business Research, University of Texas, 1967. Pp. 472. $6.00.)

Until the recent attention given to environmental variables by state and local policy researchers, the importance of demographic variables for the political system and public policies has not been generally recognized. For this reason, census data, in spite of their utility and potentially quite low cost to the researcher, have not been used. The *International Population Census Bibliographies* should further decrease user costs, and this volume on Europe should have exceptional utility for persons interested in both cross-sectional and longitudinal analyses of comparative policy research.

The volume presents an annotated bibliography by country and in chronological order of all available published censuses of the countries of Europe. This means that knowledge concerning data on not only demographic characteristics but also housing patterns, income, land ownership, and other politically relevant variables is available to researchers. Census data are available in many countries for well over 100 years and often on a state, city, and country basis. For the researcher who needs to include such variables in his analyses, the *Bibliographies* provide the best available source to indicate what can be found and where.

R. KENNETH GODWIN

*Oregon State University*

**The Civil-Military Fabric of Weimar Foreign Policy.** By Gaines Post, Jr. (Princeton, N.J.: Princeton University Press, 1973. Pp. x, 398. $16.00.)

This excellent study will challenge the notion that the change from Weimar to Hitler marked anything quite so neat as a democracy retreating before the mob in accordance with constitutional processes. Professor Post makes clear that the essential characteristics of the late Republic (1930–1933) were those of temporizing, of attempting to compromise with disaster, and, finally, of immobility. As a consequence, the surrender of an ineffectual republic was merely one more in a series of surrenders. One may wonder whether anything as neat even as surrender ever occurred. Post does not force upon his narrative a predilection for the sharp break, for what Meinecke called "the caesura," between two distinct eras in the winter of 1932–1933. It is part of Germany's tragedy that contradictory policies and opposing personalities simply fade into one another. Nothing demonstrates this better than the civil-military relations of the Weimar *system:* a "paradigm . . . [which] concentrated power in the executive-departmental sphere and stressed interdepartmental planning" (p. 189).

Dealing with functional relationships more than with actors, the book does not concern itself directly with personal vignettes. Personalities, to the extent that they are described, emerge from the political maelstrom in which they (often, of course, because they are who they are) are caught. It is only occasionally that one senses that policies (or relationships) might arise from character or lack of it. This is not necessarily a virtue, but the book thereby avoids one of the risks of historical writing: limning individuals in greater scale and sharper outlines than the evidence warrants, or else blurring an individual's distinctive qualities by suggesting that these were all of an age or of a condition. Professor Post avoids the now fashionable psychohistory; for this we are in his debt.

This judicious study provides a picture of cautious foreign and military policies dictated more by necessity than by good faith. The characteristic policy approach, as outlined by Post, was one of indirection. Germany embedded her international objectives in the policies of others; and civilians and military interchanged their domestic goals under the rubrics of revision, security and stability.

One learns that despite some attachment to Clausewitz during the Republic, underlying the relations between the military and the civilian was a sense that for Germany war was not seen as a "continuation" but as a reversal of policy, as an instrument of *another* policy. The "fabric" in the title involves the shared interest of the Wilhelmstrasse and the Bendlerstrasse that war not be excluded as a means of undoing political settlements. It remained for the political side to placate the West while both sides planned for revision of the frontiers in the East. Revision was a strategic objective, linked with security in the East; it was not, according to Post, basically a humanitarian goal.

Civil-military relations were not simply in *a* state of equilibrium, but in *the* state of heterogeneous equilibrium which is at the root of post-1919 Clausewitz. Each "weight" had its own constituencies, guiding principles, counsels, and powers. This system remained in a state of conditional armistice among its subsystems until its legacy of suspicious and tainted activities served to embolden all manner of laughing heirs. As Post demonstrates in an exceptionally lucid discussion, the fragility of the balance was shown not only when the civilians and military participated in joint planning: the tensions between them carried over into and exploited service rivalries. Post provides the ingredients of this balance—a "conditional definition of 'political leadership' and the military's selective obedience to that leadership" (pp. 351–352). Party politics and a bureaucracy politicized at the top were phenomena not anticipated by Clausewitz. Thus Weimar made explicit a flaw in Clausewitz: the assumption of a responsible civilian or political component to give direction to an inherently responsive military whose views were complementary to the political.

Military aloofness from politics, to the extent it existed, can be explained, Post correctly notes, as well by suspicion of the parliamentary system as by fidelity to Clausewitz. The attitude disappears with the ascendancy of von Schleicher, whose approach to the parliamentary system was unabashedly and quintessentially political. Versailles had succeeded in making an inner emigrant of the military. The military's hostility to parliamentarism, a sentiment it shared with the executive and the bureaucrats, then made it a readily available inner balancer between the executive and the legislature.

Basic to Post's essay is the realistic Foreign Office and Defense Ministry view that, given the behavior of the Reichstag, the priority of the political over the military should be narrowly interpreted as being within the exclusive purview of the executive and bureaucracy. Especially instructive as to the depths to which the Republic had militarily and politically sunk is Post's treatment of the drama surrounding Schleicher's selection as chancellor.

Gaines Post has laid before us the irony that the highpoint of the *Reichswehr*'s power was the selection of the "field-grey eminence," Schleicher, at the very moment he was forced to admit the failure of his long dabbling in politics. Ultimately all—civilian and military—succumbed to the chronic debility which characterized the Republic. Only the paramilitary, it seems, was spared.

The Weimar system represented a restless and laborious balance between a military weight which sought to maintain and increase Germany's physical force and regain its own *éclat,* and a political weight which sought to increase Germany's moral force and freedom of action. As the two were inextricably fused and reciprocally dependent, the failure of the one would require increased energies and genius of the other. The last man to attempt to maintain this balance was both a politician and a soldier.

Professor Post has written a book whose very excellence will rescue it from being *the* definitive work on the topic of civil-military relations and German foreign policy between the two world wars. He raises too many attractive questions and opens too many avenues for additional research for this to be the proverbial last work. And, with the modesty characteristic of the good historian, he shies from such conceits.

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**Patterns of Australian Federalism.** By J. E. Richardson. (Australia: Australian National University Press, 1973. Pp. 142. $6.00.)

The decision to establish a Centre for Research on Federal Financial Relations at the Australian National University in 1970 was unquestionably wise. One would be hard put to find a more critical and permanent area of federal study than the way monies are raised and allocated between the federal and state governments. However, the decision by the Centre to publish Professor Richardson's monograph and especially to make it the first of the proposed series of research studies to come from the Centre was most unwise. Unhappily, it is not his best work. First, the title is misleading. The 104 pages of this mimeographed text largely consist of a fragmentary commentary on such items as—the legislative machinery of the Commonwealth Parliament, Commonwealth legislative powers and their interpretation, Commonwealth-State finances, types of Commonwealth-State cooperation, the separation of powers in the Commonwealth Constitution, and the various attempts to amend the Constitution. Neither the title of these sections, nor the content suggests a "pattern," let alone "patterns" of Australian federalism. Indeed the evocative use of the title "Patterns of . . ." is too close to the box-office eyes of the film distributor who gives the title "Sabbatical Lust" to the simple story of two small children exchanging a kiss at Sunday school. Again, it is surprising to find such an outmoded arrangement and treatment of the material. One would have hoped that by now constitutional lawyers, even if they scorned pop-jurisprudence, would have left the style of the 20s and conveyed some sense of the close politico-legal mix of their subject.

The teasing question throughout, however, is, "Who does the writer intend to instruct or enlighten?" It cannot be the constitutional historian, for the historical content is incidental and far too slight. Nor can this monograph serve any student of politics, for there is little of politics, and the very little there is, when it is not transparently tendentious (e.g., p. 63) hardly takes cognizance of the distinction between constitutional law and realpolitik. Were Professor Richardson aware of the distinction he would see no paradox between the readiness of the Australian states to complain of the federal invasion of their "sovereignty" and their continuing tolerance of the power of early imperial legislation to nullify their laws. The first belongs to the pressing and permanent reality of the struggle for funds; the second is an anachronism that rarely discomforts them. Again this monograph can hardly be of any special interest to students of constitutional law. For while there are synopsized bits and pieces that would not normally appear in constitutional law texts (for example, the three brief sections baedekering instances of Commonwealth State cooperation) the rest is an analytically thin goulash of what is available in more complete and sophisticated studies of Australian constitutional law. Possibly, the novice federal politician who is totally unread in the powers of the Commonwealth may find some value in this work. For in some respects it is a bit like a handy *vade mecum* to the constitution. Possibly too, those who are drawn to constitutional revision may find some interest in the writer's proposals to amend the constitution. For example, proposals for the removal of the right of ultimate veto from the Senate, ways of extending the federal power over economic affairs, suggestions for the revival of the Commonwealth Inter-State Commission, changes to improve the process of judicial review, proposals to facilitate federal taxation on behalf of the States, and so on. There is some value in these ideas even if they are almost entirely without novelty. But to graft these recommendations on to a hotchpotch of law, microtonic slivers of history, and subliminal political messages is not what a research monograph should be about.

RUFUS DAVIS

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**Atencingo: The Politics of Agrarian Struggle in a Mexican Ejido.** By David Ronfeldt. (Stanford, Calif.: Stanford University Press, 1973. Pp. viii, 283. $10.00.)

As I was reading David Ronfeldt's excellent study, I thought of Regis Debray's thesis that a successful revolutionary process requires the establishment and development of a hard-core guerrilla force. But this did not take place in the Mexican agrarian struggle. As Professor Ronfeldt indicates, revolutionary conditions have not existed in Mexico for decades. The *ejidatarios* might have been critical of certain officials and policies, but most of them had a deep respect for the government's authority and firmly believed in the benevolence of the president. There was no legitimacy vacuum in Mexico for the *ejidatarios* of Atencingo. They chose to struggle for reform within the established institutional system, and if I might modify Debray's terminology, they offered an example of a struggle for Reform in the Revolution.

In his well-documented study, Ronfeldt chooses the period from the 1920s to 1969 to analyze the causes and consequences of political and economic struggle in Atencingo, a complex which included a town of 7,500 people, one of the principal sugar mills of the country, the ejido of Atencingo, the ejido's fertile cane lands and credit society, and eight villages in the ejidal system. The author focuses on a long series of struggles—often marked by violence—among the peasants, powerful economic interests, and state and federal government officials for control of the ejido's valuable lands and production. But rather than a description, Ronfeldt offers a comprehensive analysis of agrarian struggle in Atencingo, crystallized in an excellent final chapter, by focusing on the effects of the strategic context in which the struggle had taken place; the strategies and tactics employed by the participants; and the consequences of the struggle.

The strategic context for the ejidatarios was in part determined by the allies and opposition they had at a given time. Ronfeldt discusses the role of William Jenkins, a former American consul who remained in Mexico for more than 40 years to amass a fortune. At the time of his death in 1963, it was estimated that he was worth between 200 and 300 million dollars. It was his land that was expropriated to form the Atencingo ejidal complex. Jenkins remained a powerful figure through his control of the sugar mill and the administration of the ejido. He was a formidable force in his opposition to any meaningful structural reform; exploitation, murders and violations of the law were commonplace under his regime.

Ronfeldt points out that strategically the *ejidatarios* had very little political or economic power. On major and many minor issues, what happened in Atencingo was determined by developments in Mexico City and Puebla (the state capital). Such external realities often blunted the struggle for agrarian reform. For example, the attitude of a new state governor or president of the country was of crucial importance. Presidents Ruíz Cortines and López Mateos were more sympathetic to the aims of the *ejidatarios* than was President Díaz Ordaz, who was more interested in order than reform. The same reality held true for the attitude of bureaucrats in governmental agencies, particularly the all-important National Peasants' Confederation (CNC). Ronfeldt also feels the different perceptions of the government and the *ejidatarios* affected the effort to realize structural changes. The former wanted political stability and economic development, concentrating on sugar cane production; the latter wanted local institutional power and agrarian reform. They demanded a radical change in the ejido structure: diversification of crops, division of the ejido, parcellation of ejido land. The government did allow local institutional power, but basic structural reform threatened key political and economic interests. This was a major issue in the struggle. Ronfeldt explains how the *ejidatarios* utilized a variety of strategies and tactics in their effort to demonstrate a combination of popular support and government support. They had to cultivate governmental officials; popular support alone was not sufficient to win concessions. They did succeed at times in electing their own leaders to run the ejidal complex, but even they broke many promises and the *ejidatarios* were forced to resort to bureaucratic lobbying, propaganda, direct action (violent confrontations, work stoppages, building seizures, a march on Mexico City). As Ronfeldt indicates, however, the government effectively employed cooptation and control. It utilized the patronage system and benefitted those who cooperated. Throughout the years the government might react to local pressure by conceding local power, but it denied reform by immersing the *ejidatarios* in such technical tasks as census, survey and mapping.

Ronfeldt concludes that the consequences of the struggle were a victory for the government, to some degree the mill administration, and a defeat for the *ejidatarios*. Ultimate structural reform did not take place, although the *ejidatarios* did receive some general socioeconomic progress in their standard of living. The government retained its power and assured stability under its control.

David Ronfeldt's work provides an excellent analysis of the interrelationship of politics and economics in the struggle for agrarian reform. It also raises serious questions concerning the promises and performance of the Mexican Revolution. The book should be of considerable value to those interested in comparative institutions, the problems of developing societies and the process or revolutionary change.

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**Education and Politics in India: Studies in Organization, Society and Policy.** Edited by Susanne Hoeber Rudolph and Lloyd I. Rudolph. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 470. $20.00.)

The title of this edited volume may raise hopes that it will focus attention on one of the many important aspects of the relationship between politics and education in India. As the subtitle suggests, however, the contributions are concerned with organization, society and policy; topics that seem almost to expand, rather than delimit, the book's parameters. Though the subject matter of the contributions is wide-ranging and diffuse and their quality somewhat uneven, the serious reader should not be deterred from carefully exploring this book for the significant insights and information it contains. Some mapping of its scattered resources, however, may be useful.

The book is concerned for the most part—though by no means exclusively—with Indian higher education. Its most consistent substantive focus is the organizational character and quality of Indian educational institutions. The general theme that engages the attention of most of its authors is the permeability of those institutions to external political influence. Forty per cent of the volume is devoted to studies of the political structures and environments of three colleges—Presidency in Calcutta, Muir in Allahabad, and Mohammedan Anglo-Oriental in Aligarh—and two universities—the Maharaja Sayajirao University of Baroda and Osmania University in Hyderabad. Three other chapters explore the influence of communal interests and party politics on higher secondary schools and intercolleges in Faizabad District of Uttar Pradesh, on privately-managed schools and colleges in three districts of Mysore State, and on primary schools managed by *panchayati raj* institutions in Rajasthan.

The book's most penetrating and significant theme concerns what, in other hands, might be referred to as elements in the political culture of education in India. Eschewing social scientific terminology in favor of an analogy deriving from recent developments in biology, however, the Rudolphs suggest that the historical legacy of educational institutions in India "impresses" upon the contemporary educational system "certain forms and traditions that function rather like a *genetic imprint,* which dictates the further evolution of their institutional arrangement, not with the exactitude it exercises in a biological organism, but with a certain dependability" (p. 13, emphasis added).

A series of historical themes is developed that focuses primarily on university education. The reader becomes familiar with the early pattern of government subsidization of privately-sponsored colleges; the development of affiliating universities which functioned not as centers of advanced education but rather as bureaucratic control mechanisms designed to set examinations and maintain standards in the colleges; the bureaucratization of professors in an Indian Education Service (IES) cadre; and the growth of university governing bodies that were at once early training grounds for Indian competitive politics and heavily infiltrated by governmental representation.

In a revealing chapter, for example, Irene A. Gilbert describes both the conditions and traditions that led to the creation of the IES and the resulting patterns that shaped subsequent Indian education. The need for career incentives to attract qualified applicants to teach at struggling Indian universities was met by the establishment of a cadre that supplied not only professors but also educational administrators and inspectors. The mixing of teaching and administrative roles, the emphasis on building civil servants for the Raj and gentlemen of character, and the relatively late development of professionalization among academic disciplines in English universities all contributed, Gilbert argues, to the failure to develop traditions of academic professionalization that might have withstood the pressures toward bureaucratization to which Indian academics have largely succumbed.

A second chapter by Professor Gilbert on three of India's early privately-managed colleges describes patterns of internal conflict that developed in the early twentieth century as the nationalist movement politicized students and Indian members of the governing bodies of the colleges. Carolyn M. Elliott's chapter on Osmania details the progressive evolution of that institution from an urban and Urdu-oriented university to one more responsive to public pressures of rural Hyderabadis, and subsequently to a major university in the expanded state of Andhra Pradesh. The theme of her study is the university's efforts to maintain key elements of its autonomy in the face of increasing challenges from the state government.

The University of Baroda is better prepared to resist the threats to the autonomy of its academic interests than most Indian institutions. A unitary, rather than affiliating, university, it has traditions of disciplinary autonomy and excellence. Its early links with the elites of Baroda were maintained as that group established itself in prominent positions in both state and municipal post-independence politics. The Rudolphs, in a lengthy chapter on this university, document the evolution of its management institutions in response to the increasing politicization of its environment. They argue, on the basis of extensive data, that while the university has resisted pressures for the "localization" of its student body, it has accommodated demands for "easy access professional and occupational training at more modest standards" (p. 237).

Related features of the genetic imprint are drawn on by Harold Gould to explain the accelerated exploitation of higher secondary schools and intercolleges in Uttar Pradesh by local notables as institutional bases for their political ambitions. Traditions of the nationalist movement leadership in sponsoring educational development and government grant-in-aid programs to support private institutions, Gould argues, combined in the post-independence period to make such institutions both accessible and rewarding bases for partisan organizations. This theme is effectively illustrated by case studies drawn from the author's field research in Faizabad District.

In these detailed descriptive studies, and in the first three of the Rudolphs' six introductory chapters, the reader gains a number of important insights into the political and bureaucratic roots of contemporary higher education in India. The authors write from the perspective and with the insights of political scientists. At the same time, their concern for the intellectual growth and integrity of Indian universities is apparent. Their consequent sensitivity to the often conflicting values of the university as an autonomous center of learning and the university as an instrument of democratic change is one of the appealing features of this book.

The quality of the other studies varies greatly and often, it seems, inversely with their relevance to the central themes of the book. Paul Brass has contributed a very interesting chapter on the highly effective political lobbying efforts of the revivalist Ayurvedic medicine movement. Brass describes how this group pressured the central government to grant it a large measure of professional legitimation and support for a massive Ayurvedic educational establishment. Analyzing the internal conflicts that have plagued the movement in its efforts to develop coherent medical practices, Brass concludes that, "The failure to develop adequate educational and professional standards in the Ayurvedic colleges has meant the production of thousands of new, poorly qualified practitioners, if not quacks, providing a low quality of medical relief to both urban and rural residents" (p. 371). The study might more appropriately have been included in a volume on the politics of medicine in India, but it makes rewarding reading nevertheless.

The chapter by T. N. Madan and B. G. Halbar uses caste and religious data from 156 privately-managed schools in Mysore to show that the students and staff in these institutions are disproportionately grouped in schools managed by members of their own communities. Many readers may be less surprised by this finding than by the diversity of students in these schools: fully 60% of the students served by these sectarian institutions are from communities other than that of the management of the school they attend. In any case, one must share the Rudolphs' skepticism (p. 88) of the authors' contention that the social composition of these schools is convincing evidence that they "fail in a crucial manner to advance the modernization of the educational system and society" (p. 146).

Iqbal Narain, in a chapter on primary schools in Rajasthan, argues that the transfer of control over those schools from the state government to local political institutions of *panchayati raj* has "infused politics into the management of primary schools" (p. 162). He cites Legislative Assembly debates as evidence for partisan political activities of teachers, political motivations in personnel and distributive decision-making, and deterioration of standards under *panchayati raj.* His contentions are marginally strengthened by questionnaire data drawn from a sample of 85 respondents in 2 of the 232 *panchayat samitis.* The Rudolphs, in establishing a comparative context for the study, suggest the contrast with decentralization in New York City: "teachers in Rajasthan are subject to being used or appropriated by community or partisan interests, while teachers in New York . . . seem to have appropriated the educational system for their own benefit" (p. 90). In attributing this difference to weaker teacher professionalization and bargaining capacity in Rajasthan, they miss the key structural distinction: functionally specific political bodies such as school boards are much more subject to capture by professional interest groups than are general purpose bodies such as panchayats.

Despite the weaknesses and idiosyncratic subject matter of some chapters, this book does, then, have much to offer. Its first three chapters, especially, provide a lucid and insightful analytic introduction to the study of education and politics in India. These pages, and selected portions of the rest of the volume, should be read by anyone concerned with this subject, and particularly by the scholar interested in beginning to fill the many gaps in our knowledge and understanding of the complex relationship between the political and educational spheres in India.

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**The Heroic Image in Chile: Arturo Prat, Secular Saint.** By William F. Sater. (Berkeley: University of California Press, 1973. Pp. 252. $10.50.)

Here is a haunting book and, sadly enough, a most timely one for students of Latin America and for Chileans alike. This work of an historian is an outstanding contribution to the understanding and appreciation of the taproots of Chilean political culture and the impact of historical idiosyncracies on present-day politics.

All societies develop specific needs and desires to create heroes in their own image. A time of societal crisis, the uncertainties of change processes, and the ambivalence of individual or group response to the challenges of the day all tend to focus the societal spotlight on the hero. The hero as the martyr, the hero as the avenging angel of real or imaginary injustices, the hero as the "political saint." On May 21, 1879 a Chilean naval officer, Arthuro Prat became a symbol of national commitment, rejuvenation, and dedication to act and to sacrifice. Alas, his sacrificial act was *per se* a meaningless one, the culmination of a meaningless battle in a meaningless war. Or so would claim the cynics.

Indeed, Prat's simple act of heroism had no pragmatic basis, nor did it significantly alter the course of Chilean history. What it did, though, was to provide a touchstone for judging the attitudes and behavior of Chileans of later generations. The eminently pragmatic, and for a typical *latino* much too unself-conscious, Chileans became both haunted and fascinated by the existential figure of an authentic if not also enigmatic hero of their historical consciousness. But just what exactly did the image of this naval officer represent for Chileans of the past and the present?

As Professor Sater most convincingly argues, the constant resurgence of Prat's popularity and his adulation is clearly related to the cycles of internal political, economic and societal crises Chilean society experienced. The anxiety and the uncertainties of the processes of change compelled Chileans constantly to reinterpret the specifics of Prat's behavioral relevance to their age and their circumstances. The hero singlehandedly captures the images of nationalism and readiness to act instead of debating, and compromising—qualities so much cherished by the urban *chileno.*

These qualities of course directly relate to the questioning of one of the most revered institutions of political bargaining in Chile, the parliamentary system! Time and again Chileans under the stress of change wondered if the endless bickering and the impotence to act in the Chilean congress is something that a country can really afford.

Similarly, Prat's image tended to remind Chileans that their country in many respects is merely nominally independent as long as the dominant economic interests in society tend not only to link themselves to the outside world but also to ape its social mores and consumption habit. This clearly carries an eerie connotation of memories of things recent past.

The two-year lifespan of the socialist experience and the bloody intervention of the military in 1973 all find some explanation in this brilliant exposé by Professor Sater. And explanations also give rise to some nagging doubts about decades and volumes of historical and political analysis that have dissected Chilean political behavior and institutions. Were they all to the point in stressing the inherent stability and strength of institutions, the "correctness" of the military's behavior and the relaxed and sophisticated tone of political bargaining? I believe that it is in exactly this realm that the lasting value of Sater's work lies. In light of the implications of his exposé, students of Chilean politics will have to reexamine some of the basic assumptions we now hold about Chile's political culture. Bless the strength of the persistent social historian who can place the unpleasant reminders of the past into the proper conceptual perspective for us political scientists!

For those of us who will have to return to our "homework" on analyzing the Chilean political system, this volume offers an excellent documentary and historical backdrop. The use of new materials is extensive and the bibliographic material included is a most comprehensive one. Professor Sater clearly offers us an opening salvo for a "revisionist" reevaluation of many things we assumed as given. For that alone his contribution is more than appreciated.

Arpad von Lazar

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**Burma: A Selected and Annotated Bibliography.** By Frank N. Trager. (New Haven, Conn.: Human Relations Area Files Press, 1973. Pp. xii, 356. $18.00.)

The explosion of publications, documents, articles, monographs, and dissertations—has confused as much as clarified our knowledge of non-Western countries. That Burma, one of the world's least visited states, should yield 2100 entries in a substantial bibliography is a comment on the inflation in quantity of publications. Still, the task is incomplete, for although the volume includes some 96 dissertations, it omits all but 50 Burmese language references including the most important journals; furthermore all non-Western language sources are excluded. For the researcher who uses German, French, and Japanese there are other English bibliographic sources noted, in addition to a list of nearly 200 Russian and other Slavic entries.

Although there are brief annotations which move the novice a step past the titles, there is no critical comment to suggest the relative importance of material; however, cross-referencing will aid those who proceed by discipline. For example, "politics" and "administration" are separate categories in the topical index, and authors are indexed both alphabetically and topically.

The value of *Burma* for the individual scholar may be questioned if a university is nearby, for the price is $18. Most of this material is listed in the Association of Asian Studies annual bibliography (which also includes Burmese journals), the Cornell and Ohio University Southeast Asia (and Burma) bibliographies, and the standard *Readers' Guide* and *International Index*. The *Guide* and *Index* will be a means of updating Trager's entries which end in mid-1970. While the book is handy, it may be too expensive a luxury for any but the most avid researcher, therefore it should be purchased by any library with a pretense of Asian coverage to assist the individual scholar.

While scholars have complained about the paucity of social science work on Burma, this collection reveals a mass of material that elevates Burmese studies well beyond the "exotic." Burma escaped the deluge of third-rate social science financed by the Department of Defense for Indochina and Thailand. Many of Trager's references are of the highest quality, in particular the historical research of J. S. Furnivall and Gordon Luce which ranks with the world's best, while the anthropological research of Manning Nash and Melford Spiro is superior to most U.S. scholarship elsewhere in Southeast Asia. Unfortunately such critical commentary is not part of this bibliography. Another gap in our knowledge of Burma is revealed in the lack of humanistic sources. While Burmese Buddhism has long fascinated the West, other literature has yet to be seriously analyzed. Since neither Professor Trager nor any other American-born scholar can really use Burmese easily, it remains for the next generation of students to fulfill the promise of "understanding Burma" which can only come through a command of humanistic studies.

John Badgley

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**Abuse of Psychiatry for Political Repression in the Soviet Union.** By United States Senate, Committee on the Judiciary. (New York: Arno Press, 1973. Pp. 257. $10.00.)

This volume of personal testimony and documentary materials is a reprint edition of the record of a hearing held before the U.S. Senate Judiciary Committee's Internal Security Subcommittee on September 26, 1972. At the hearing, testimony was offered by the logician, poet, and mathematician Alexander S. Yesenin-Volpin, of the faculty of Boston University, who has been incarcerated in Soviet psychiatric facilities on five occasions as a result of his activities as a political dissenter. He was permitted to emigrate in the summer of 1972. His final incarceration was prompted by his application for a U.S. visa when he was invited to an American university to attend a scientific meeting; his release was effected as a result of a protest by ninety-five Soviet scientists, most of whom were from the University of Moscow and subsequently suffered reprisals.

The bulk of the hearing materials consists of documents most of which (eight cases) were gathered by Vladimir Bukovsky, himself a prisoner, who appealed to Western psychiatrists in 1971. There are additional materials from *samizdat* sources, including a report by Zhores Medvedev and an open letter by Aleksandr Solzhenitsyn.

One quarter of the volume deals with Major General P. G. Grigorenko who taught at the Frunze Military Academy and who is one of the more prominent Soviet political dissenters. The documents include the text of the speech he was to deliver before a Tashkent court in defense of the deported and persecuted Crimean Tatars—one of the activities that led to his incarceration. Judged on the basis of his own statements and writings, Grigorenko hardly appears to be a mentally ill person. Two commissions of psychiatrists disagreed in their findings regarding Grigorenko. The Tashkent Commission, which examined him for three hours on August 18, 1969, found him to be in good mental health; the other commission, at the pliable Serbsky Institute of Forensic Psychiatry in Moscow, met with him for twenty minutes on November 19, 1969 and advised that he be incarcerated. Included in the volume are open letters of Mrs. Zinaida Grigorenko describing the abuse and mistreatment that he is said to have experienced at the Serbsky Institute and in a military prison in East Prussia—characterized by her as "slow murder."

The volume contains personal accounts by a number of detainees as well as the texts of reports of psychiatric commissions, letters, protests, and appeals. The official reports and other materials provide evidence regarding the kinds of "symptoms" of "illness" relied upon by some Soviet psychiatrists. These include: "behaved with an over-high opinion of himself, without any self-criticism," "obsessive fears and persecution mania," and "reform delirium." Two detainees were asked why they had beards. Some of the alleged acts of deviant behavior that led to detainment and incarceration include the Red Square demonstration of August 25, 1968 (Nataliya Gorbanevskaya and Viktor Feinberg), active propagation of religious belief (Gennady Shimanov), recording one's philosophical views regarding alternatives to Soviet rule (Vassily Chernishov), distributing one's own satirical verses (Anatoly Ponomarev), attempting to leave the country by illegal border crossing (Vladimir Shleshnev and Anatoly Chinnov), defending General Grigorenko (Vladimir Gershuni), "anti-state utterances" (Vladimir Borisov), publishing abroad (Boris Yevdokimov), and signing protests (Leonid Pliushch).

Soviet psychiatry emerges from these document with a tarnished and compromised reputation. Hasty examinations and superficial "diagnoses" result in excessive use of "schizophrenia" or "chronic schizophrenia of a paranoid type" to describe the condition of detainees. Able to identify "psychopathic personalities" by fiat, certain Soviet psychiatrists have become auxiliaries of the security police, willingly identifying new "syndromes" that require hospitalization. Political "patients" and detainees have no means of appealing the decisions of medical commissions. Injections of drugs given to prisoners are often detrimental to their health; prisoners are also threatened on occasion with being placed among severely disturbed patients. The prisoner becomes a "cured patient" if he renounces his views. The sculptor Mikhail A. Naritsa has summarized Soviet political psychotherapy very succinctly: "The main method of treatment is intimidation. The main symptom of healing is repentance. The treatment may last as long as they wish. One can always say, 'he is not well yet.'" (p. 189).

The Soviet leaders can undoubtedly provide a rationale for these practices. With the connivance of unethical psychiatrists, political dissidents can be incarcerated without a public trial or judicial proceeding of any kind and cannot challenge the finding of mental illness. Dissidents can thus be deprived of the opportunity to speak critically using the political trial as a tribune. Dissidents can be given debilitating drugs under the guise of "therapy," and it can also be reasoned that the dissent movement will be discredited more readily if enough of its advocates are certified as "mentally ill" after being silenced by essentially administrative action.

The materials in this volume make for depressing reading. Apart from the human suffering conveyed in the personal accounts and the abuse of governmental authority, there is the abdication of professional responsibility and abandonment of medical ethics by certain Soviet psychiatrists and other medical personnel. The personal accounts on psychiatric practice and on conditions in Soviet mental hospitals—especially those by Grigorenko, Shimanov, and Gorbanevskaya—indicate low professional standards. It is a dubious achievement to equate political dissent with mental disorder. One is prompted to ask: Is this creative development of a new tenet of Soviet Marxism-Leninism?

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**Laos: War and Revolution.** Edited by Nina S. Adams and Alfred W. McCoy. (New York: Harper & Row, 1971. Pp. 482. $15.00.)

This volume, published under the auspices of the Committee of Concerned Asian Scholars, offers varied fare in its 30 chapters and nearly 500 pages, ranging from thoughtful, well-researched essays to trivia. Written while the U.S. was deeply involved in the raging war in Laos, the book was politically aimed at condemning U.S. intervention. This theme pervades most of the articles, giving consistency to the collected contributions, and marks the book as an example of the prodigious efforts mustered by many young Asian scholars to alter U.S. policy in Indochina. Despite this admirable effort, it must be noted that a number of the essays were perishable when first written (for example, "A Liberated View of the War: American Businessmen, Laos . . . and Beyond"), and even the most thoughtfully researched have a measure of contentiousness which reduces their value for those who are searching, as the passions of war diminish, for more sober evaluations.

Several background articles dealing with the land, the people, and the historical development of Laos are very good. Two French anthropologists, Condominas and Moréchand, offer useful brief ethnographic profiles. John K. Whitmore's thoughtful chapter on the Thai-Vietnamese struggle for Laos in the 19th century illuminates both the historic Vietnamese concern over hostile forces manipulating the upland populations of Laos who spill over into the hills of North Vietnam, and the Thai concern that the Mekong Valley territories bordering Thailand remain in friendly hands. These historic observations remind us of the importance of a Thai-North Vietnamese accomodation on Laos for attaining regional stability. A well-written essay by Alfred McCoy on French colonialism in Laos from 1893 to 1945, drawing abundantly upon French sources, makes the point that the French viewed Laos as a pleasant backwater, and shows how little effort they devoted to the development of the country. This light French intervention, and the resultant slow pace of modernization in Laos, compared to its Vietnamese neighbor, help to explain the slower growth of nationalism in Laos. An interesting essay by Nina Adams which explores patron–client relations reveals how the Lao elite, recognizing their weakness compared to more powerful external powers, have linked themselves to outside patrons, the French, Japanese, Thai, Vietnamese, and Americans. As this article reaches the contemporary scene, however, objectivity fades and the central theme obtrudes. Adams suggests that the Vietnamese have been good patrons while the Americans have been bad for their Lao clients. Although it is true, in this reviewer's opinion, that the tragic U.S. intervention in Indochina has had disastrous effects upon Laos, the North Vietnamese intervention has not proved an unmixed blessing. Unfortunately, the North Vietnamese role is not adequately analyzed anywhere in this volume.

More than half of the book is devoted to an evaluation of U.S. intervention in Laos. Two seriously researched chapters in this section, one by Jonathan Mirsky and Stephen Stonefield and the other by D. Gareth Porter, persuasively document and condemn the escalating, if clandestine, U.S. role in the conflict. Fred Branfman, in a forceful article aimed at exposing U.S. mischief in Laos from 1964 to 1970, diminishes the thrust of his arguments by trying so hard to minimize the North Vietnamese presence in Laos during this period. Most of the authors of this book see little distinction between U.S. policy from 1954 to 1960, when the U.S. manipulated internal Laotian politics in favor of right-wing elements, and the post-Geneva 1962 period, when the centrist tendency of Souvanna Phouma was supported. The authors show little sympathy for Souvanna whom they portray as becoming a bogus neutralist by 1964, joining with his former right-wing antagonists under powerful U.S. pressure.

In view of the sympathy of this book for the Pathet Lao—Noam Chomsky characterizes them in the introduction as the only viable nationalist political force in Laos—the section on the Pathet Lao is the most disappointing. Of a total of six chapters on this subject, two are verbatim interviews of a Lao refugee and a PL Director of Information in Hanoi, conducted by Chomsky on flash visits to Vientiane and Hanoi, with little background on the interviewees and no analysis of their observations. Another verbatim interview of a North Vietnamese party official, reported by an Australian anthropologist, could, like Chomsky's interview of the PL official, be matched by a random selection from translations of Hanoi radio at the time. The section is rounded out by a translation of a PL peace plan, a journalistic account of life in the PL zone, and—the most interesting—a short memoir of a Vietnamese adviser to Prince Souphanouvong in 1945. This thin assortment hardly justifies the promise of the subtitle that one of the themes of this book will be "revolution."

Despite its shortcomings, and particularly in view of the paucity of literature on Laos, this book will reward the careful reader with interesting information and opinions.

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**The Nutrition Factor: Its Role in National Development.** By Alan Berg, portions with Robert J. Muscat. (Washington, D.C.: The Brookings Institution, 1973. Pp. 290. $8.95, cloth; $3.50, paper.)

As political scientists become increasingly interested in policy issues arising out of development, works such as Alan Berg's *The Nutrition Factor: Its Role in National Development,* become increasingly important. The discussion and analysis of public policy cannot take place in a vacuum. Whether we are analyzing the politics of health programs in the United States or issues of development internationally, we need to have some insight into the relationship between the underlying technological and biological factors and the administrative programs that emerge to deal with grave social and economic problems.

The world problem of food and malnutrition is becoming much more pressing. Malnutrition is widely conceived as an obstacle to national development and, especially among children, is a cause of impaired physical and mental development, with adverse effects on education, productivity, and the working life span. Moreover, the complexity of the factors involved is such that in the short run, improvements in nutrition may cause an increase in birth rates, but in the long run improved nutrition may very well turn out to be a precondition for effective population control.

Berg, who prepared this study under the auspices of the Brookings Institution and the Foundation for Child Development, is a nutritionist who worked in India on nutrition problems and is now associated with the World Bank. The discussion, which reflects Berg's experience and concern, covers issues ranging widely from technology to the politics of nutrition. There are interesting chapters on malnutrition and development, nutrition and the population dilemma, agricultural advances, problems in education for better nutrition, the crisis in infant feeding practices resulting from the decline in breast feeding, fortification of foods as a solution, the potential contributions of private industry and public programs, lessons from the Indian experiment, and a broad discussion of policy directions and program needs.

Although only two subsections of chapters are specifically labelled as dealing with politics—The Politics of Feeding Programs (pp. 176–179) and Nutrition and Politics (pp. 192–194)—much of the book deals with issues relevant to public policy analysis. The concluding chapter on policy direction and program needs is particularly provocative.

The link between American domestic needs and foreign policy approaches to food distribution is very briefly but incisively explored. Child feeding programs, for example, are described as "politically attractive, for they are direct, highly visible, and generally well received" (p. 176). These programs have had their origin in "relief efforts motivated by compassion, political attraction, and the need of affluent countries—primarily the United States—to dispose of surpluses" (p. 177). Our preoccupation with assuring that the donor of food is identified is shown to have caused antagonism and to reflect in part a lack of understanding of how nations are perceived in underdeveloped countries. One authority is quoted as saying "Some children who have learned that their daily school milk comes from America have told us that America is the name of a cow" (p. 177).

Berg appropriately notes that in the development of nutrition programs "at a certain stage, nutrition problems move away from the domain of technology toward the domain of administration" (p. 191). Among the problems of administration are issues of budgeting, management, logistics, and marketing but also political concerns such as resolving conflicting interests and promoting support. Another political issue is the widespread failure to build in adequate evaluation processes in a wide variety of public welfare programs. In part, reluctance stems from a fear that a concession of shortcomings in a program would jeopardize future funding.

Implementation of successful nutrition efforts is particularly troublesome. Many elements are involved including agriculture, health, education, and industry. Berg explores whether to fix administrative responsibility for nutrition programs on an interministerial council, on an existing ministry, or on a separate agency and tends to favor a separate agency. Councils, he says, have proven to be inffective; and an existing ministry, where nutrition programs may be under the control of vested interests "is unlikely either to give support or to have the breadth necessary for an effective nutrition program (p. 204).

Berg's book could not be more timely. With a new situation emerging out of recent changes in patterns of international food sales and problems of new areas of drought in such places as Africa, international food and nutrition policies require reexamination. Alan Berg's book is highly recommended as a fascinating study of the relationship of technical, organizational, and political problems in developing world nutrition policies. It is a major contribution to the necessary redirection of these policies and programs.

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**Czechoslovakia Before Munich: The German Minority Problem and British Appeasement Policy.** By J. W. Bruegel. (New York: Cambridge University Press, 1973. Pp. xiv, 334. $14.95.)

**The Slovak Dilemma.** By Eugen Steiner. (New York: Cambridge University Press, 1973. Pp. ix, 229. $13.95.)

The common theme of both works is not just Czechoslovakia, but more specifically the nationalities problem of that country. Professor Bruegel's book, first published in Germany in 1967 under the title *Tschechen und Deutsche* in a somewhat differing version, is a logical continuation of Elizabeth Wiskemann's classical treatise of the same title, published in 1938. It not only goes beyond that date chronologically, but represents a distinct improvement because the author was able to draw on diplomatic documents published recently, including many Czech sources.

Again, the book is useful in presenting an abbreviated but cogent history of Czech political aspirations under Austria-Hungary and a lucid exposé of the first Republic's political and economic development. On the whole it gives a very favorable picture of that experiment and of President Masaryk's efforts to build a state that would be democratic, multinational, progressive, and humane. It points out that the German minority was gradually being won to constructive cooperation with the result that "until 1935 no parliamentary elections in Czechoslovakia ever showed less than two-thirds and sometimes as much as four-fifths of the German voters adhering to parties with a positive approach to the State" (p. 33).

In addition to Czechoslovakia's internal development, the author discusses its external relations with Germany. Documents subsequently available from German archives reveal some interesting facts, such as the continued efforts of German envoys to Czechoslovakia to improve relations and to play down German nationalist ambitions. Thus, even Eisenlohr, appointed in 1936 by the Nazis, "confronted with the realities, . . . became a kind of advocate of the Czechs in Berlin" (p. 151).

British appeasement policy has been described by others, but it becomes even more misguided when read in the framework of Czech efforts to compromise and "appease" the Sudeten Germans. Perhaps one weakness of the book is that it makes no effort to analyze the sources of British behavior at that time. Surely the blame could not have been attributable solely to Chamberlain and his closest collaborators. There are, however, interesting sidelights: Vansittart who later embodied anti-Nazism is shown to have been on the side of appeasers of Henlein and Hitler before Munich.

The book ends, as indicated by its title, at Munich. It does not deal with the final chapter of Czech-German internal relations—the expulsion of three and a half million Germans from Czechoslovakia in 1945 after some seven hundred years of cohabitation and, only occasionally, confrontation.

Steiner's work deals with the other half of the country and specifically with Slovak nationalism. It has the advantage of carrying the analysis up to the present, and much of the book is devoted to what may be called nationalism under communism. It is indeed, as Professor Steiner tells us, "a paradox that nationalism should have played so significant a part in the development of a socialist society" (p. 2). Literature on Slovaks and Slovakia is rather meager in the West, and Steiner's brief outline of the position of Slovaks under Hungary which lasted for almost one thousand years, as well as its history from 1918 when they united with the Czechs in a common state (except for the brief existence of an independent, or at least separate, Slovak state between 1939 and 1945) is very readable. His most original contribution to our knowledge, however, is his description of the role of Slovaks since the Communist takeover in 1948. The author writes from experience as an active party member until 1968 when he left for England.

People may differ about the real significance of the Slovak national uprising in 1944 and some aspects of it are still a bit cloudy. The book does present an interesting account of the interplay between the government-in-exile in London, the Communist exiles in Moscow, certain elements of the Slovak state government in Bratislava and the grass-roots local underground.

Perhaps the most suggestive chapters deal with the Slovak roots of the Czechoslovak Spring of 1968. It may be true that "the Czechoslovak Spring of 1968 was preceded by the Slovak Spring of 1963" (p. 112), when a cultural protest movement erupted in earnest in Bratislava. From then on the several streams united: in the author's terms: "Slovak liberal nationalism and Czech national liberalism" (p. 120). Gradually the two took more distinct shapes: the Czechs wanted more democratization, the Slovaks more self-government. The story of an obscure Slovak *aparatchik* who more or less involuntarily became the symbol of liberalization in the person of Alexander Dubček, and the transmutations of a prisoner of the Stalinists into a tool of Brezhnev in the person of Dr. Husák is well told. It is ironic that the only remaining feature of the 1968 Spring is the federalization of Czechoslovakia and the creation of two republics, Czech and Slovak. Even that achievement is blurred by the fact that although the government has been federalized, the Communist party is as centralized as ever, and perhaps more-so, because the Secretary General, Husák, is a Slovak himself.

Underlying both books is the universal problem of integration and separation, especially on the ethnic level. Of the two contemporary tendencies, that of global fusion and of national fission, the latter seems the more prevalent. If nations and nation states have trouble sticking together, the cause of global (or regional) unification does not seem to have much chance.

FRANK MUNK

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**Patrick J. Hurley and American Foreign Policy.** By Russell D. Buhite. (Ithaca, N.Y.: Cornell University Press, 1973. Pp. xiv, 342. $14.50.)

With the proliferation of historical and biographical studies of American diplomacy during the Second World War and early Cold War it was only a matter of time before historians turned their attention to Patrick J. Hurley. As Franklin D. Roosevelt's personal representative to Chiang Kai-shek and subsequently as ambassador to China, Hurley played a prominent role in the evolution of American policy toward China in the critical years 1944 and 1945 and was a central figure in the postwar recriminations over the failure of that policy. In this biography Professor Buhite analyzes Hurley's role in the "China Tangle" while illuminating the less-known aspects of a career as soldier, politician, and diplomat.

Professor Buhite traces Hurley's life from his "frontier origins" in the Oklahoma Territory through a prosperous law practice and service with the American Expeditionary Force in World War I, to prominence in local politics. His early support for Herbert Hoover led to a position as Secretary of War in which role he secured national attention for his suppression of the Bonus Army March in Washington in 1932. Exceeding his instructions to protect the business district, he directed the Chief of Staff, General Douglas MacArthur, to drive the protestors from the city and destroy their encampments. Criticizing Hurley's actions, the author observes that he misunderstood the plight of the veterans, overreacted in his use of force, and misled the public by emphasizing the radicalism of the demonstrators. The author is equally critical of Hurley's role in the controversy over independence for the Philippines. As the chief spokesman against independence, the Secretary of War reflected a paternalism and nationalism reminiscent of nineteenth century imperialism, leading Professor Buhite to conclude that the Oklahoman was insensitive to the aspirations of Filipinos and the political undercurrents running through Asia.

In the 'thirties, Hurley returned to his law practice, representing the Sinclair Oil Company in important negotiations with the Mexican government. Although a Republican, he maintained political and personal ties with the Roosevelt Administration and, after Pearl Harbor, served as the President's personal representative in the Southwest Pacific, Middle East, and Russia. His most important assignment was China where he pursued a policy of compromise and accommodation between the Kuomintang and the Communists. Historians have characterized Hurley's approach as inflexibly anti-Communist. Professor Buhite, in a lengthy analysis of the China mission, argues that the envoy accepted the need for military unity in China and conscientiously pursued accommodation. He demonstrates that, at least in the early months of his tenure, Hurley was neither irrevocably committed to the Kuomintang nor particularly anti-Communist and rightly observes that the policy of support for Chiang Kai-shek reflected the exigencies of war and the personal preferences of both Presidents Roosevelt and Truman.

An envoy, however, is more than a passive instrument of policy; his reports and recommendations often determine policy. This was Hurley's greatest failure and the source of his celebrated feud with the young China specialists on his staff who sought to reverse the open ended commitment to Chiang's regime. In his attempt to be fair the author obscures the fact that Hurley's suppression of his staff was, at best, impolitic and, at worst, a scandalous abuse of his responsibility to analyze objectively the political situation in China. The author's criticism of the foreign service officers for bypassing the ambassador and ignoring established policy is rather gratuitous given the facts that events demonstrated the bankruptcy of the established policy and (as both Barbara Tuchman and John Paton Davies have written) that Hurley abused his staff, refused to consult with his specialists, and rejected contrary advice and information.

In general, Professor Buhite offers an informed and objective analysis of a rather enigmatic figure, and his research reflects extensive use of the Hurley papers and other archival sources. This is a competent biography and a useful addition to the literature of recent American diplomacy.

David J. Alvarez
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**Russia in the Caribbean, Part One.** By the Center for Strategic and International Studies. (Washington, D.C.: Georgetown University, 1973. Pp. 36. $1.00, paper.)

This monograph, based on a Caribbean conference held in October, 1971, under the auspices of the Center, is primarily of a prescriptive nature, which consists of a set of policy guidelines. The panel suggests that the postwar era of American predominance came to its end in late 1960s when the Soviet Union acquired an "effective and credible" second-strike capability and began to launch an active diplomatic, military and economic expansionist policy throughout the Third World. In the Caribbean, this policy is spearheaded by a naval expansion, carried through two distinct but interrelated strategies. The Soviets attempt to improve the strategic balance through the deployment of a nuclear submarine force out of Cuba against the soft underbelly of the U.S. strategic defense system. What alarms the panelists more than this, however, is a 20th-century Soviet version of gunboat diplomacy, a policy aimed at influencing the general political environment of the area by diplomatic overtures, naval demonstration, anti-American propaganda, and economic assistance.

Against these perceived realities, the panel suggests several policy guidelines, which may be characterized as a revised version of the Roosevelt corollary to the Monroe Doctrine. According to the panel, the U.S. should (1) stiffen her reaction to the Soviet expansion by resisting hostile communist and *other* anti-U.S. regimes; (2) seek an "understanding" with the Soviets for the mutual scaling down of naval activities in the Caribbean and the Black Sea; (3) normalize the U.S.-Cuban relations through a negotiated settlement of outstanding problems and reintegration of Cuba into the Hemisphere system; and (4) observe a conditional noninterventionist policy provided that a Caribbean regime does not receive a direct support from the hostile military power or attempt to subvert a pro-American regime. Such a noninterventionist policy, it is proposed, should apply to a regime which may adopt a different political system or ideological orientation "as long as they play a constructive role" (p. 22).

The gist of the policy recommendations is the proposal that the U.S. maintain a policy of nonintervention against any indigenous political movement which does not solicit Soviet politico-military support. This approach, it is said, is necessary to accommodate, rather than resist, rising nationalism. Yet it must be understood that the new nationalism is essentially an economic movement directed against nonindigenous economic forces, i.e., the United States private investors and traders as well as their local economic allies. Favorable responses to Soviet overtures by the new nationalists are natural consequences of the détente and their desire for economic sovereignty, a condition which would easily render the panel's theoretical differentiation either impractical or incredible.

Hoon M. Chung
*Illinois State University*

**The South West Africa/Namibia Dispute: Documents and Scholarly Writings on the Controversy Between South Africa and the United Nations.** By John Dugard. (Berkeley: University of California Press, 1973. Pp. 584. $28.50.)

John Dugard, advocate of the Supreme Court of South Africa and Professor of Law at the University of Witwatersrand, has put together a work of nearly 600 pages dealing with the South West Africa/Namibia dispute. Although he provides a capsule description and a brief history of this rich, sparsely populated territory, the central thrust of the book is an extensive analysis of its history since South West Africa became a mandate under the League of Nations.

Carefully weaving official documents and scholarly writings, Dugard covers the political and legal developments from that time to the present, ending with his personal suggestion for a resolution of the dispute. His conclusion is straightforward enough: "If the parties involved are genuinely concerned about self-determination and the best interests of the people of South West Africa, or Namibia, rather than the promotion of their own ideologies, this [a joint UN/South African sponsored plebiscite] surely is the best course" (p. 542). Dugard is most eager that this plebiscite be "free," and he calls for a series of stipulations, including the freeing of all Namibian political prisoners and their forthright participation in the plebiscite (which would be held one to three years in the future in order to ensure maximum understanding of the choices by the electorate).

*The South West Africa/Namibia Dispute* is comprehensive but not complete and a South African cast inevitably creeps into some of the analysis. Is the dispute really "the international *cause célèbre* of the century" (p. xi)? Do the Khoi Khoi and San people have to be referred to as the Hottentots and the Bushmen? Does not the author's reliance on scholarly and official documents automatically undercut the positions of the Ovambo People's Organization and the later South West Africa People's Organization? Would the South African government really accept the prerequisites of a "free" plebiscite?

Still, when all is said and done, there is a great deal of material here, most of it well presented. Subsequent scholarly work in this area will have to wrestle with its conclusions and analysis, even though other authors may want to challenge many of its conclusions and its overemphasis on official sources.

CHRISTIAN P. POTHOLM

*Bowdoin College*

**Religion and Soviet Foreign Policy 1945–1970.** By William C. Fletcher. (New York: Oxford University Press, 1973. Pp. 179. $11.25.)

Professor Fletcher seeks to survey an aspect of Soviet foreign affairs previously neglected, "the role of religious organizations and individuals in Soviet foreign affairs" (p. 1). Religion's primary role, he avers, has been in molding public opinion abroad, image-building, and the "development and use of a vast array of non-propaganda activities designed to elicit a favorable reaction to the USSR, whether generally or in specific matters" (p. 2). Such a role has been essentially marginal in terms of the entire array of Soviet foreign policy activities.

The main success of religious activity abroad has been in the exploitation of the peace issue, particularly during the Korean and Vietnamese conflicts when Western opinion was receptive to such appeals. Fletcher describes this process in his chapter on the Prague Christian Peace Conference, a meeting in which a combination of an issue with broad appeal to many circles of society, a parallel Soviet policy goal, and an absence of direct Soviet involvement or dictation redounded to the Soviet Union's benefit. *How much* the CPC strengthened Soviet influence on the peace issue is left vague.

Fletcher is most satisfactory in describing philosophical antinomies between Soviet politico-religious mechanisms and religious doctrines abroad and analogous dilemmas at home. Islam, he contends, reduces distinctions between the sacred and profane, and even a Communist atheist from Soviet Central Asia may enjoy advantages in the Muslim world of the Middle East because in the Muslim tradition, the political-religious linkage is an advantage. The Muslim emphasis on social justice and the "righteous society and state" offers advantages to a power emphasizing these themes. Buddhism, on the other hand, is pessimistic about temporal existence and lacks an ethic of social improvement. This makes difficult any application of "broad theories of social aspirations" (pp. 74, 82). (Adda Bozeman's pioneering work on multi-cultural approaches to international law also exemplifies this type of analysis.)

Fletcher's discussion of the philosophical-political dilemmas of church and regime is also cogent. He asks how the Soviet regime can use religious themes and benefit from religious instruments abroad without strengthening religion and religious-based nationalism at home, and points out that the Islamic emphasis is particularly pertinent in this connection. How can the Russian church assist the regime's foreign policy purposes abroad, ostensibly to assist in the preservation of its slim institutional structure at home without undermining any remaining vestiges of independence or weakening its domestic base of support? How can it desire simultaneously to promote certain values and support regime foreign policy goals and survive a crisis like Czechoslovakia, particularly where brethren in Eastern European churches are involved? The dilemmas of "concordat" politics are not unique, of course, to the Soviet system, but the "contradictions become more acute" in a regime that officially proscribes religion.

Fletcher's command of his subject is less apparent in his discussion of specific questions of foreign policy and international relations, partly because of the inevitable difficulty in defining "influence" abroad. Thus, "influence" is at times reduced to the negative goal of using religious themes and organizations abroad without suffering undue disabilities from simultaneous religious persecution at home. There is also the ubiquitous "image building." Fletcher's description of the "continuing penetration of the Middle East" as one of the "most spectacular successes of the post-Stalin era" (p. 68) can hardly be accepted without qualification. The Soviets seem to be as much "the hunted" as "the hunter," alternatingly beguiled and bedeviled by the Middle East "sub-system." Nor can one accept his sharp distinction between "the actual situation" and "the current ideological assessment" in describing Soviet studies on Muslim areas before the middle 1950s (p. 73). Is there truly a "value-free," "objective" analysis of the Middle East, or was the Soviet analysis simply marred by ideological distortion? Fletcher offers no guidance on this question. He concludes that the Church deserves a share of the credit for helping to build a more favorable image of the USSR in the West (p. 95) without saying why it deserves such plaudits.

The work will be of value to students of Soviet "church-state" relations who have also benefited from Professor Fletcher's other works. It has something of interest to say on the "clash of cultures" question and contains some interesting observations on Soviet relations in Eastern Europe and the church role there. For those concerned with Soviet policies in the Third World, especially the Middle East, the work will be of less value, and it does not add much, in my opinion, to our existing store of knowledge about Soviet relations with the West.

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**Confrontation and Accommodation in Southern Africa: The Limits of Independence.** By Kenneth W. Grundy. (Berkeley: University of California Press, 1973. Pp. xxi, 360. $12.50.)

If in the aftermath of the Portuguese coup of April 25, 1974, Mozambique becomes independent under a black government, will there be a domino effect in Rhodesia, Angola, Namibia, and eventually South Africa? Could an independent Mozambique afford to stop the export of its labor to the Witwatersrand, deny to Transvaal rail traffic the port of Lourenço Marques, and do without South African tourists? With the fall of white Rhodesia, is it likely that the Western powers will move toward an even more overt support of South Africa than now exists? What are the strengths, the weaknesses, and the prospects of white domination in southern Africa?

Professor Grundy's book contains a good deal of data pertaining to these questions. The author believes there is an urgent need to understand southern Africa's "inherent potential for violence" (p. xi) and the dangerous involvement of states outside the region. The final two pages of his concluding chapter express both "pessimism and optimism" about the future of white domination (pp. 300–301). This discussion begins with the judgment that "the situation, currently, is moving in a direction beneficial to the white governments," but after an appropriate warning that "over-pessimism" may be "as unwarranted as was the euphoria" of the late 1950s and early 1960s, Grundy concludes that "the southern African sub-system seems to be, for many reasons, a fundamentally unstable one."

There is much to praise in Grundy's skillful and comprehensive work, the first book-length survey by a political scientist of the southern African regional subsystem. The book should also be of interest to political scientists who are engaged in developing systemic theory in their study of international relations and "regional sub-systems," a term which Grundy prefers to " 'subordinate' state systems."

But if it is fair to judge the book as an analysis of the strengths, the weaknesses, and the prospects of white domination in southern Africa, the book is a disappointment. For this I blame the author's preoccupation with systemic theory. In seeking conceptual clarity, he focuses on the region and the discerning of intraregional patterns. In analyzing his data, he asks how a particular "patterned interaction" fits into the currently functioning subsystem, and describes and categorizes it, rather than asking how the data bear upon the survival of white domination. The domino theory, for example, is referred to, but its validity for southern Africa is not directly examined, although Grundy is not averse to futurology, as shown in his *Guerrilla Struggle in Africa* (1971). For the most part, despite identification of trends, the book is curiously static.

Indeed, one wonders how useful is the focus on the region since it excludes the giving of important attention to (among other extra-regional actors) the United States, Britain, and other Western powers that support the white-governed states of southern Africa. Focusing on interstate relations within the subsystem also underplays the importance of the political and economic structure of power within each state, and the probable effect upon that structure of major changes outside it.

Over half of the concluding chapter (and much of the theoretical appendix) is concerned with problems of defining the southern African "border," "membership" in the subsystem, and criteria for delimiting it. Fortunately, Grundy's realistic good sense enables him to surmount his theoretical preoccupations. For example, he recognizes the sterility of the "boundary" issue, noting that in southern Africa policies and not boundaries lead to conflict. The book itself proceeds from the outset, though somewhat apologetically, to define southern Africa "arbitrarily . . . in broad geographical terms" (p. xiv): South Africa and Namibia (South West Africa); Rhodesia (Zimbabwe); the Portuguese territories of Angola and Mozambique—all ruled by whites—and the black-ruled states of Botswana, Lesotho, and Swaziland, linked to South Africa in a customs union; the Malagasy Republic (Madagascar); and (north of the Zambezi) Malawi, Zambia, Zaïre, and Tanzania.

Grundy is keenly aware of the difficulties facing the researcher whose interest is southern Africa. Although concerned that his study may be "less than methodologically rigorous, at least in terms of modern political science," he points out that "the unavailability of comparable over-time data" makes an eclectic approach necessary (p. 303). One may add that adequate field work is also difficult to accomplish, and access is sometimes impossible (for example, to South Africa, which denies visas to some American academics while the United States grants visas to all South African academics who apply). Grundy gathered data during visits to southern Africa, including South Africa, in the late 1960s. Through careful use of press and documentary sources, his writing is generally up-to-date. One lapse is the lack of recognition of Botswana's mineral wealth and potential for development.

Of particular interest is his description of South Africa's economic hegemony and dynamic thrust into the region. Based upon this are economic, transport, military, and diplomatic networks—and flows of labor—that serve largely to bolster the *status quo* and to place constraints on neighboring black states. The book also describes the international relations and problems of guerrilla activity, the generally futile efforts of a few black states to "build bridges," and South Africa's so-called "outward-looking" policy toward black Africa. This policy, now in decline, is well described as one that strengthens *apartheid* by sowing confusion among some African states, improving South Africa's image among Western policy-makers, and encouraging wishful thinking among South African whites.

Southern Africa deserves higher priority, including scholarly priority, in the United States. It is hoped that Professor Grundy's book will contribute to redressing the balance.

THOMAS KARIS

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**The Dominican Intervention.** By Abraham F. Lowenthal. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 246. $10.95.)

The international crisis which resulted from the revolution and near civil war in the Dominican Republic in 1965 was quickly overwhelmed in the minds of many scholars and policy makers by the simultaneously escalating developments in Vietnam. Few scholarly book-length treatments have appeared, though articles and chapters of books have been numerous. Now a slender volume has come from Abraham F. Lowenthal, who lived in Santiago, D.R., throughout the entire crisis period and thus developed numerous resources unavailable to others.

Lowenthal has chosen to make a case study of the process of foreign policy making up to May 3, 1965, a date when the intervention in the revolution by the U.S. became a full-scale military as well as political operation. Within its limited scope (a matter to which we shall return), the work is on the surface a balanced and insightful presentation of valuable historical data, and a very fine analysis of the process which produced the U.S. intervention, in which Lowenthal sheds much new light on developments in both Santo Domingo and Washington. Beneath the surface, however, lurks a problem of methodology common to many studies of decision-making in national security affairs: the use of many private and secret documents and interviews without attribution, and in some cases without *any* identification.

Of about 150 persons interviewed, some of them intensively, eleven refused even to be listed in the appendix, and most of the others talked on a "background" basis. What the author refers to as "a great deal of written material" (p. vii) is paraphrased and unidentified as well. Lowenthal comments that this approach was "the price for obtaining a great deal of previously unavailable information" and one the reader will have to share. He is aware of the consequences and issues that this raises, and assures us that he gave much thought to the problem, especially after reading Theodore Draper's essay on the subject. The author states, however, that he must stand by his original commitments. The problem, therefore, remains fundamentally unresolved.

Lowenthal minimizes its effects by judiciously balancing conflicting points of view and by his use of such terms as "reportedly" and "apparently" as signals to the readers. The author, however, is not primarily a journalistic reporting an "inside" story, but a scholar presenting a book with a thesis which belongs in the realm of science. His colleagues are ultimately unable to determine the validity of that thesis without access to much of the data upon which it was based. Since Lowenthal restricts the thesis to the Dominican situation and makes no claims about its applicability to analogous situations, the problem is further amplified.

Assuming that the data presented are as complete, balanced, and objective as possible (they certainly appear to be, based upon their relationship with information already known and their own consistency and coherence), we may then consider the author's hypotheses concerning the foreign policy-making process. He begins his analysis by pointing out the weaknesses of the three prevailing schools of thought: the "official," the "radical," and the "liberal." Their common and most fundamental failing is reliance upon a "rational policy model" which evaluates actual behavior in terms of clearly stated and understood goals. In summation of his own thesis, Lowenthal states that actions are shaped by "perceptions and information, by priorities and procedures of different actors in the policy-making process, by bargaining and politics, by external pressures and internal strains, by conflict resolution and consensus building. Competition, compromise, coalition, and confusion combine in uncertain ways to produce occurrences we term foreign policy" (p. 149).

Lest we infer that he concludes that no real thesis is possible, i.e., that neither rules nor predictability apply, the author quickly adds that the actions in the Dominican case were the result of a definite pattern of attitudes, processes, and behavior with fundamental and identifiable causes. He points out three: (1) a set of assumptions about the environment, in this case the location of the Dominican Republic, the character of its political and military leaders, the threat of a "second Cuba," and the undesirability of obvious intervention in the internal affairs of a Latin American republic; (2) a "tendency to decide as little as possible at each stage of the crisis and to postpone difficult choices" (p. 157); and (3) a tendency to "act on the basis of extremely optimistic appraisals and predictions" about the situation (p. 159), meaning the position of our "friends," of course. These factors conspired to result in the massive intervention which came four days after the crisis began.

The thesis and its probable applicability to other crises seem almost beyond question. I can even see its relevance to understanding the behavior of the Nixon administration in the "Watergate" situation. While its very simplicity is a mark of its validity, some will argue that it lacks theoretical substance and rigor, or that it explains nothing because it attempts too much. Certainly the thesis quite properly accounts for much uncertainty and unpredictability about specific actions in this or analogous cases. It would seem, however, that scholars utilizing the thesis will be able to discuss probabilities of certain courses of action, and to understand why some events do or do not occur. The question still open concerns the balance between predictable and unpredictable factors in any theory of political behavior.

A final criticism, however, relates to the problem of the data. The limitation on the period covered leaves the vast number of subsequent developments untreated, and thus unrelated to the author's thesis. This is especially regrettable in the case of this crisis about which much remains unknown, and about which Lowenthal seems to have much new information. Those of us who are Latin Americanists or students of international politics, and who have focused on the Dominican crises and the many actors involved in its eventual settlement, would have to agree that, as good as the data available from documents and other books are, Lowenthal's treatment of the subsequent developments would have been very useful.

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**China and America: A Bibliography of Interactions, Foreign and Domestic.** Compiled by James M. McCutcheon. (Honolulu: University of Hawaii Press, 1972. Pp. 75. $4:50.)

Professor McCutcheon's bibliography vastly underrates the existing knowledge of Sino-American relations. It is a disorganized mixture of antiquated and recent or still useful works. Frequently an author's major contributions are omitted and some minor article cited. Basic monographs published years ago are listed as "unpublished doctoral dissertations." Topics vital to an understanding of Sino-American relations are ignored, and no effort is made to relate Sino-American relations to the world and domestic events of which they were essentially an integral part. The most positive feature of the bibliography is its strong emphasis on Chinese communities in the United States. But here it suffers from competition in a field of great current popularity in which many similar bibliographies are being produced.

DOROTHY BORG

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**Conflits et Coopération entre les Etats, 1971: Prélude à un nouvel ordre international.** Edited by Jean Meyriat. (Paris: Armand Colin, 1973. Pp. xiv, 285, 63 F.)

In this volume, produced by the highly regarded *Centre d'études des relations internationales* of the *Fondation nationale des sciences politiques,* thirteen French scholars have contributed a wide array of essays that are linked by the common purpose of analyzing the mechanisms of either a major and recent episode in international relations or the behavior and motivation of a leading protagonist. Although the authors largely confine their analysis to events of the year 1971, the editor, Jean Meyriat, justifies this on the grounds that 1971 marked a watershed between the dominant postwar bipolar East-West pattern of international relations and a new international order of multipolarity and North-South conflicts. The claim of special significance for 1971 merely indicates another candidate for the title of "transition" year, a designation awarded and contested annually since at least 1963. Nonetheless, the current configuration of international relations does differ in significant respects from the dominant postwar pattern, and M. Meyriat can point to major developments in 1971 which constituted important shifts; the admission of China to the UN and the beginnings of a thaw in US-Chinese relations, major changes in US foreign policy, Britain's successful negotiations for entry into the European Communities, Brandt's Ostpolitik, and events in South Asia.

Each of the contributions has been prepared specifically for this volume: despite the divergent perspectives of the authors, most of the selections—all in French—are very high in their quality of description, explanation or analysis, and some of them are provocative. Jean Meyriat, who is Director of Research at the *Fondation,* notes in his introductory essay the prevalence of major international conflicts in areas formerly under colonial domination and then concentrates on the increased relevance of the North-South conflict. He justifies his analysis by stressing the growth of US-Soviet solidarity and finds that the two superpowers share a common interest in extinguishing local wars or at least in preventing their spread. But Meyriat's assumption is incomplete, since it does not take sufficient account of other interests to which the US and USSR accord higher priority (cf. Soviet behavior in the Middle East prior to and during the October 1973 war).

One of the most provocative essays is that of Marcel Merle, whose thesis is that a new world equilibrium is leading to a heightening of internal tensions and a crisis of the nation-state. In particular, Merle sees three sets of international pressures—national rivalries, transnational movements, and global environmental constraints—all tending to paralyze national leaders' power of decision. While Merle gives excessive credence to the "Report from Iron Mountain" on the difficulties of coping with peacetime, he does provide convincing arguments about the dangers of defensive, nationalist, and isolationist reactions as each of the developed states tries to protect itself from international resource, economic, and monetary constraints. In essence, the decreased salience of Cold War tensions has allowed internal problems to become more manifest; whether the consequences may be as grave as Merle fears is another matter, but his is a broad-gauged and provocative essay.

Another contribution, that of Manuela Semidei, provides a useful foreign perspective on the Nixon Administration's foreign policy. Semidei is critical of President Nixon for denouncing isolationism, yet taking unilateral acts reminiscent of 1930s isolationism, and she notes effectively that moral and political irresponsibility can isolate the US as much as any putative unreliability. The virtue of Semidei's analysis lies in her focus upon the US problem of rationalizing its foreign policy with the lessening of its global anti-communism; certainly this helps to explain why bridge-building to the USSR and China has sometimes coincided with a worsening in allied relationships.

Of the remaining essays, several provide incisive and highly detailed analyses of individual conflicts. C. Tirimagni-Hurtig offers a skilled and well-written contribution on "The End of Equilibrium in South Asia: The India-Pakistan War and the Victories of Mrs. Ghandi," in which she rebuts the idea of Indian expansionism and stresses the self-defeating nature of US policy in the region. Pierre Gerbet and Françoise de la Serre contribute detailed, balanced and useful discussions of Britain and the European Community: Gerbet's, an analysis of the negotiations of 1971; de la Serre's, a discussion of the British domestic setting. The book also contains an excellent chapter on

South America (by Alain Rouquié), a discussion of French foreign policy (Roy Macridis), and essays on the admission of China to the UN, the Soviet role in the Third World, tensions in the Arab world, details of the Berlin accords, and a striking set of geopolitical graphs.

Any generalization about this collection must be limited because of the divergent subject matter. The editor's contention that the authors share a common conviction that it is possible scientifically to analyze contemporary social and political phenomena is neither developed nor demonstrated in the individual chapters. Nonetheless, and despite the fact that some of the analyses have been outdistanced by events, the contributions are of generally high quality and basically carry out the authors' stated objective of picking out the present tendencies in IR and recognizing the direction in which these are evolving.

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**The Next Phase in Foreign Policy.** Edited by Henry Owen. (Washington, D.C.: The Brookings Institution, 1973. Pp. 345. $8.95, cloth; $3.00, paper.)

Perhaps if the Department of State had maintained a full-scale policy planning function instead of scaling it down in 1969, the present collection of planning papers would have been produced routinely within the Department, rather than by the shadow government of Brookings. (I am not being entirely fair, of course; some basic conceptual thinking about U.S. foreign policy pre-1969 was done outside the government by one Professor Henry Kissinger.) But it is not surprising that Henry Owen, Chairman of the last full-fledged Policy Planning Council, should have produced this policy-options-and-forecast document outside the Department.

This book addresses questions essential to mapping the U.S. role: How will the world change? Will the U.S. accommodate to that change? How? The book organizes itself rather like the Department itself, discussing seven "regional trends" (Japan, Western Europe, East Asia, Middle East, Latin America, Soviet Union, and China); functional areas of economics, general purpose forces, and strategic arms; domestic opinion, and new forces in world politics.

In his brief curtain-raising essay Mr. Owen, correctly in my opinion, argues that established premises must be questioned. (It will not be unfair to evaluate the book's fidelity to that injunction.) No consensus has yet been formed on the next phase, except that the United States should do less and rely on others (but not the Soviet Union) to do more. On the one hand the nation-state will function quite differently in the future; on the other hand it is still the best mobilizer of resources, and in the less developed countries represents a driving force.

Thus "new trends generate new needs and problems," but "these must be confronted in the context of ancient institutions and attitudes. An old epoch is dying, but a new one has not yet come of age" (p. 8). Others have also concluded that, in the words of Matthew Arnold, we are suspended between two worlds, one dead, the other powerless to be born. But the needed transformation is elusive to the bureaucrat who mistakenly believes he can control events, and to the theorist who overreacts to transnational phenomena and prematurely writes off governments as the prime actors.

The essays follow the editor's prescribed approach and thus have a kind of coherence not always found in collections. (Most sound as if they were finished in 1971; it is a shame they were delayed in publication.) The advantage they share is in being private-sector counterparts to balanced, prudent in-house analysis. The disadvantage they labor under is that by never thinking the unthinkable, they lose that extra dimension of imagination and boldness of planning that is free to challenge first-order assumptions rather than operate confined within the preconceived framework.

Morton H. Halperin's essay on Japan is thus sound, at least for the time when it was written, but it may not be able to help us very much if circumstances substantially change. The Okinawa Reversion and the decline of the American-Asian presence "makes the [U.S.-Japanese security] treaty seem less necessary" (p. 12). My own 1974 conclusion was that the shifts that have taken place in trade and energy sectors make the stability of the strategic-military environment more essential than ever.

Halperin's review of the main issues is sensible but a little dated; the nuclear option, so widely discussed no more than a half-dozen years ago, is no longer seriously talked about, and NPT ratification may be more likely than before. The issue of American bases is now dormant and, as with Western Europe, the personalized style of Henry Kissinger's diplomacy increasingly decides whether the nagging nonstrategic issues can be contained or will escalate. I agree completely with (and preached all over Japan in 1974) the virtue of multilateral routes to minimize some of the frictions arising in Japan's foreign presence.

John Newhouse produces a sound diagnosis of the weaknesses of NATO and Western Europe. His skepticism regarding French cooperation stands up in the light of French strategy of *sauve qui peut* during the oil crisis of 1973-74. Newhouse is skeptical of a Western European *entente nucléaire* and sees the present European security system as probably more cost-effective than any likely alternative. He may well be right, and my only quarrel is that he confines his vision to the near future, which deprives us of one comparative advantage of independent policy analysis.

Ralph N. Clough in his essay on East Asia seems to me too complacent and accepting of U.S.-China policy for the twenty-year period preceding the Nixon-Kissinger turnaround. He unwittingly confirms one's sense that U.S. policy noticed the Sino-Soviet split a lot later than it should have, and still tends to see Asia as "theirs" or "ours." But he makes an appealing case for a Southeast Asian neutral zone.

In one of the best chapters Robert E. Hunter writes prophetically about a more neutral United States acting as mediator in the Arab-Israeli confrontation. His perceptive analysis of the situation stood up very well in 1973–74. It is also one of the few contributions touching on the sociology of the region. Given the latter's implications for longer range U.S. policy, I wish the editor had included that dimension in his authors' checklist.

The essay on Latin America by Peter T. Knight and John N. Plank is first-class. It realistically recommends "creative pluralism" (p. 107) and sensibly calls for "constructive disengagement" (p. 108). It would have been interesting if the authors had considered a scenario in which the U.S. disengages from the OAS and operates vis-à-vis a less neurotic hemisphere, much as the U.S. is going to have to do in Western Europe.

With his customary gusto, Professor Brzezinski advances the theses of inherent Soviet instability (a "metastable state," p. 114) and intra-bloc contradictions that so endear him to the Kremlin. He also calls for a Western military edge for bargaining purposes. His optimism regarding détente is thus very restrained, but over time he sees it as demanded by the interdependencies of the worldwide "metropolitan political process" (p. 132).

Finally, A. Doak Barnett produces a characteristically thoughtful piece on China in a short-term perspective. Taiwan—the residual thorn in Peking's flesh—is increasingly forgotten as a prime issue, but Barnett usefully reminds us of it and recommends an "open-ended" U.S. policy (p. 151).

Turning to the functional areas, Edward R. Fried's piece on foreign economic policy is unsurprising. He assigns top billing to U.S. relations with Canada, Japan, and Western Europe, and looks to monetary reform as the key to developmental assistance. East-West trade he regards as nice but not decisive. One wonders if the enormous dislocation in foreign exchange ratios as a result of the fourfold increase in oil prices in 1973–74 will seriously affect his projections.

Leslie H. Gelb and Arnold M. Kuzmack restate the conventional wisdom regarding U.S. forward deployment, but leave unexamined whether the U.S. does in fact have the "responsibilities" they take as given. They usefully confront—and deny—the Fulbright thesis that military assistance invariably leads to unwanted political and strategic involvement, and their conclusion is that the U.S. should not reduce by more than one division.

Jerome H. Kahan likewise advocates a conservative strategy towards nuclear weapons, involving the air-sea-land triad, across-the-board redundancy, and the thinkability of nuclear war-fighting scenarios. With the exception of the latter, these are difficult to refute as practical counsels at any given time, but since they differ in no wise from Pentagon analyses, they are not particularly intellectually challenging.

Only in Seyom Brown's brief chapter are the "new forces at work" in the world actually named. Despite the editor's wise introductory remarks about change, only here is found any significant mention of the vast influence of science and technology on future world politics, including the increasingly politicized role of the biosphere, the transformed meaning of the seas for both economics and strategy, and the phenomena labeled as transnational. In this sense the structure of the book reflects what is wrong with governmental approaches: most officials work on traditional areas, while disconnected special assistants work on the "esoterica" that in fact increasingly influence traditional relationships.

In his introduction the editor anticipated three common conclusions: First, there has been a shift from the East-West focus to advanced nation relationships, with some North-South interaction. Second, economic issues increasingly "transcend" security issues. Third, these changes call for redirection rather than retraction of American efforts, as well as a new multilateralism to supplant the recent American role. In his concluding chapter Mr. Owen repeats that the age of nation-states, at least in the advanced world, is drawing to a close.

I am afraid that the book, with the exception of Brown's chapter, does not either act on this insight or generate speculation that would illuminate (or refute) it. There is little evidence that policies adequate to the kind of change the editor speaks of are even considered.

Its generally high quality nevertheless makes it useful to the contemporary affairs student, and its consistency with official perspectives makes it a viable substitute for policy planning papers in the tradition of the former Policy Planning Council. The scenarios are surprise-free and, if the Spenglerians among us are right, the book misses some of the main points about the future flagged by its editor. Still, it will have to do until the U.S. government finally confronts its policy planning weaknesses, and until both planners and scholars acquire the second sight I unreasonably expect of them.

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**Toward the 21st Century: Education for a Changing World.** By Edwin O. Reischauer. (New York: Alfred A. Knopf, 1973. Pp. 195. $5.95.)

Professor Reischauer's latest book is a strong, simple, and uncomplicated statement of the need for a global outlook based on the premise that "the world is becoming a single unit for mankind" (p. 75). Not only are parochialism and narrow nationalism obsolete for the times; an education which sustains them from generation to generation is an anachronism. Hence education for world community.

Yet Reischauer has no illusions that universalism will uproot localism in the next decade or two. If change comes it will occur with a new generation now embarking on primary and secondary school education. A restructured educational system must prepare the young to cope with universal problems such as the food-population crisis, worldwide environmental issues and overall demands of a world community, not by adding a course on Asia or Africa to the existing curriculum but by throwing the spotlight on mankind as a whole.

Reischauer knows how difficult this will be. Everywhere he looks communities less than worldwide are in trouble despite being drawn together by the forces of interdependence. Their problem is not their needing one another but breakdowns in mutual understanding. Writing of the new trilateralism, Reischauer warns "the real question is whether Europeans, Americans, and Japanese are ready to live with one another in such a community" (p. 126).

What is it that stands in the way? Military and economic problems as well as growth and change are one aspect of the problem. The root cause, however, is differences in attitudes, outlooks, and traditions. Doubts, suspicions, and misunderstandings result. The Japanese treat shock diplomacy or new economics from the west as somehow bound up with the West's sense of superiority. "The Japanese are . . . conscious of racism in others because they are so profoundly racist themselves" (p. 127). But Americans are not without guilt. When a balance of payments problem threatened the American economy, Toyota somehow appeared more menacing than Volkswagen, Japanese textiles caused more uproar than Italian shoes, and Japanese economic strength is widely attributed not to efficiency and hard work but to exploitative low wages. For their part, the Japanese are equally obtuse in recognizing the economic problems of the United States. "Nursing their anxieties about the outside world and living in relative cultural isolation, they have proved imperceptive to the viewpoints of other countries . . ." (p. 123). And these deep-running misconceptions between Japan and the United States symbolize the deep gulf between the developing and developed world. They are the foremost obstacle to building the kind of world community in which mankind can resolve its most urgent human problems.

Reischauer's call for world community is welcome therefore, at a time when the number and quality of strong idealistic statements has shriveled. His proclamation is not without difficulties, however, and the analysis hesitates and falters at certain crucial points: First, while he acknowledges his limitations in primary and secondary education, he quickly goes on to propose more far-reaching changes in national educational systems than any country has achieved. One thinks of the slow and painful efforts to introduce more biracial material into American education and asks how far more fundamental changes in education for world citizenship are to be achieved not in one country but throughout the world.

Second, Reischauer has little patience with discussions of the balance of power, which he consigns to the eighteenth and nineteenth century. He shows that bipolarity in great-power relations is past and concludes that talk about a five-power balance today does more harm than good. His mistake is in confusing particular expressions of the balance of power with its necessary and inevitable manifestation in every political system. Checks and balances are a form of balancing power within the American constitutional system; other forms prevail internationally whether on a worldwide or regional basis. It is illusory to suppose that international order can be maintained in the absence of ever-changing patterns of equilibrium among states. Statesmen must recognize these patterns and strive to formulate policies that channel international cooperation along practical lines given existing power realities.

Third, Reischauer asserts that the doubts and misunderstandings between Japan and the West foreshadow the conflicts likely to arise between the Third World and the West in the future. In one sense, his comparison is valid, but the example of Japan may also be misleading. Japan enjoyed a period of economic and cultural isolation denied to present-day emerging nations. Japan is rich in economic advancement and they are poor. Japan has reconciled a rather eclectic pluralistic system of values drawn together in Buddhist universalism with modernization. It is problematical whether others will be able to do this.

Fourth, Reischauer finds the grounds for world community in new economic and security factors, ignoring the fact that the moral foundations for community no longer exist. Cultures in an earlier era were united by consensus on religious, philosophical, and cosmological beliefs, as when Europeans were drawn together by a universal Catholic Church and the Latin language, and the same was true of coherence in the Muslim and Buddhist worlds. A firm cement of common faith and belief held each of these areas together, but that cement has long since crumbled. It may be that a new cement will take its place at the level of fundamental values. If Professor Reischauer is to make the case for world community, he should address this issue which is less tangible than economics or security but more compelling than new educational and curricular strategies. It is in a word discovering new moral ground for human solidarity assuming the old grounds have been weakened or passed away.

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**U.S. Foreign Policy and Peru.** Edited by Daniel A. Sharp. (Austin: The University of Texas Press, 1972. Pp. xxv, 485. $10.00.)

The papers in this volume on relations between the United States and Peru were prepared in 1969 and 1970, under the auspices of the Adlai Stevenson Institute of International Affairs, The Chicago Institute of Foreign Relations, the Johnson Foundation, and others. Their presentation before government officials from Peru and the United States, as well as academic and business people from both countries, reflects an attempt by a "citizens' constituency" to revive the relationship between Peru and the United States. Relations that were already cool deteriorated following the Peruvians' seizure, after a 1968 army coup, of a Standard Oil of New Jersey subsidiary and the sugar holdings of W. R. Grace and Company. Sharp intended his collection of articles to be particularly valuable to policy makers and specialists. Some of the contributors, such as Luigi Einaudi and William Mangin, had previously published on Peru. Even though the papers were prepared during the first months of revolutionary rule in Peru, essential elements in the strained relations—the Standard Oil and W. R. Grace cases—were not among those considered separately in this volume.

In his illuminating article on the armed forces, Einaudi pleads for a greater understanding of the political role of the Peruvian military establishment. He would have us believe that it does not fit any American stereotype of the traditional military. Einaudi states (p. 26) that many of the uniformed officers "may be more favorable toward social innovation" than the Aprista party, long considered the champion of nationalist revolutionary change in Peru. He proposes a "sensible military policy" toward Peru (p. 49), one which would be limited to the maintenance of "decent working relations on a technical basis" with the Peruvian armed forces. His conception of a desirable relationship includes unrestricted—as opposed to selective—sales of military equipment, the virtual elimination of grants, some training in military matters, and a curtailment of the military mission program in Peru.

David C. Loring contributes an indispensable analysis of the fisheries dispute. This outstanding paper is the only one followed by a critique, provided in this case by Luis E. Llosa, a former foreign minister of Peru. It is impossible, says Loring, to consider the fisheries dispute without handling the matter of Peruvian jurisdiction and sovereignty over the two-hundred-mile zone of contiguous waters. The complexities of the problem are presented by Loring: need for a regime regulating both use and exploitation of the seabed, ocean floor, fisheries, surface of the water, and also the air space immediately above. Loring suggests that the United States examine empirically all assumptions governing present U.S. policy on fisheries and maritime limits. None of these assumptions he states (p. 113), have been conclusively demonstrated to be valid. Llosa, on the other hand, recommends a series of very specific concessions by the

United States, in return for some general "compromise solutions" on the part of Peru (p. 124).

According to John Strasma, the United States should reject a "neutral" policy on Peru's agrarian reform, for it would reinforce pre-reform differences in power and income (p. 198). Strasma believes that even under the existing state of relations between Peru and the United States, positive, but "low profile," assistance would be accepted by the land reform program. Aid should be proffered either through multilateral lending agencies or by way of tangential programs, such as marketing facilities, he maintains.

The aforementioned lending agencies, themselves, have become controversial. John P. Powelson ably examines the charge that development assistance agencies were slowing down their aid to Peru because of pressure from the U.S. government. Powelson concludes that the slowdown was a consequence of the separation of powers, with the administration defending the lending agencies from the wrath of the U.S. Congress, which in turn was under pressure from the American business community because of the petroleum confiscations and fishing boat seizures in Peru. Powelson warns that a U.S. policy based on promoting the interests of specific groups of its own nationals would not be likely to encourage Peru to behave according to international standards (with respect to contracts, governmental continuity, and international law).

In a well-reasoned article, Bruce Blomstrom and W. Bowman Cutter urge the Peruvian government to delineate clearly the bounds for foreign investment. Once limits existed, Blomstrom and Cutter would insist that Americans investing in Peru live within them. Critical of the results of assistance that the U.S. government had given private investment in the 60s, the authors ask that, for its own good, the American government endeavor to remove itself from the investment process. This suggested removal of the U.S. government from the investment scene is predicated upon their hope that international agencies would have an expanded role in the resolution of conflicts arising from the presence of foreign investments.

William Mangin, an anthropologist, after taking note of the portents, maintains that "there is ample reason to expect a strong separatist movement [of Quechua- and Aymara-speaking Indians] in the Andean region" (p. 230). Mangin observes that past U.S. policy toward Peruvian Indians has been one of favoring and assisting their integration, which he feels is "a most unlikely possibility." The distinguished anthropologist advises that in the event of a separatist movement, disengagement from both the Indians and the Peruvian government would be an appropriate policy for the United States to pursue.

Labor relations are overemphasized in this volume. Neither William J. McIntire nor William A. Douglas, in an accompanying article, anticipates what has become a pervasive concern of Peruvian labor elements. Unionized labor now charges that the revolutionary government intends to replace traditional labor unions with newer, revolutionary instruments, namely its "industrial communities."

The book also contains a lengthy contribution by Robert G. Myers, on an appropriate assistance role for the United States in Peruvian education. In a peripheral article, Dan C. McCurry propounds policies for the regulation of U.S. church-sponsored activities in Peru.

Even though the statements in this volume are no longer new, U.S. foreign policy toward Peru has changed little since the provocative papers were presented. Peru has turned to the Soviet Union for deals in military hardware. The fisheries dispute awaits further universal development of the law of the sea. Private investment has not returned to earlier levels. Therefore the contributions retain much of their currency for policy makers and have lost none of their value for concerned academicians and students.

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**Basic Documents of African Regional Organization.** Edited by Louis B. Sohn. (Dobbs Ferry, N.Y.: Oceana Publications, 1971–72. 4 vols. Pp. 1854. $35.00 each.)

This is a monumental four-volume collection of virtually all the fundamental documents governing institutional relations among groups of African states. It also places the current arrangements in historical perspective by providing relevant antecedent agreements, and by giving a systematic, analytic commentary of events leading up to each arrangement. Finally, the volumes present exhaustive bibliographies of books, articles and, most important, documents themselves—considerably more than can be reproduced in full, even in this exhaustive set. All these useful tools are arranged in accordance with a schema which takes a bit of getting used to, but is helpfully explained at the beginning of Volume 1.

The volumes cover the origins and evolution of the Organization of African Unity from 1958 forward, African Development Bank, French regional groupings (with separate sections for the general African and Malagasy, the equatorial and the West African arrangements), West African, East African, and Maghrebian regional structures. They vary in importance and subject matter from customs unions and river regimes to locust control networks. In the final volume, African associations with the European Community are set out and discussed, agreements frequently characterized not as regional but as inter-regional or even anti-regional.

It is extremely difficult, even (especially?) in Africa, itself, to have access to all the legal documentation which is so essential to the official and the scholar. The days when African colonies published and sold their laws through the colonial agents' offices in London are largely past and, in the hurly-burly rush to understaffed independence, most African foreign and attorneys general's offices have had to assign very low priority to systematic collecting, indexing, binding and revising of their legal heritage. All the more useful, then, is this remarkable effort by a nongovernmental scholar. Unlike so many other collections, this is by no means merely another siphoning into one container of materials quite readily available in many other places.

Ultimately, the usefulness of Professor Sohn's and Oceana's admirable and patient venture will depend on whether it can be kept up to date—perhaps by a supplementary volume once a decade—and whether it can be paralleled by similar endeavors for other regions.

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**Change and the Future International System.** Edited by David S. Sullivan and Martin J. Sattler. (New York: Columbia University Press, 1972. Pp. ix, 109. $8.00, cloth; $2.95, paper.)

—"It does not appear possible, at least not within the next 30 years, that some new technological device will serve to neutralize nuclear weapons" (p. 10).

—"Likely technological developments, in a time frame of 10–20 years are going to generate important performance demands on the international system going well beyond, in scale and intensity, the requirements placed on the system today" (pp. 59–60).

These predictions typify the concerns of the six chapters in this short volume. The articles all deal with factors that will increase the demands for governance in the international system during the next 10 to 20 years. Each, however, examines the impact of different phenomena. Moreover, the predictions stem from quite different methods, ranging from historical analogy to implications drawn from the relationship of key concepts in equation-like formulas.

The chapters by Brodie, by North and Choucri, and by Skolnikoff deal with the impact of technology on international politics. Bernard Brodie reviews several concepts concerned with prediction, and then considers the impact of nuclear weapons and population growth. Nuclear weapons, Brodie argues, will remain a stabilizing factor in international politics reducing the likelihood of any kind of war between major powers. With respect to the population explosion, Brodie finds the trends "inevitable and worsening" but unlikely to lead to international wars because the problem is most severe for impoverished nations.

Robert North and Nazli Choucri take a different and possibly conflicting perspective on the population problem as it relates to technology and resources. They propose that a society's population and its level of technology combine multiplicatively to produce demands for resources. When a society's indigenous supply of resources is small compared to its population and technological skills, it exerts "lateral pressure"—usually some form of imperialism. Thus, according to North and Choucri, although the population explosion generates demands everywhere, it is from the technologically advanced societies that the growth leads to international problems.

Eugene Skolnikoff introduces the reader to an array of technological issues having international political impacts—weather modification, oceans, outer space, natural resources, food, and population. Weather modification receives the greatest attention. In his opening paragraph Skolnikoff argues that technology has exaggerated national sovereignty, proliferated small and weak states, and allowed the "traditional" approaches of governments to international affairs to work a little longer. He concludes that most of these effects will have to be reversed in the future.

Herbert Dinerstein uses historical analysis as a predictive base through extrapolation to the future role of ideology in alliances from its function in the 16th and 17th centuries. The rise of Protestantism and nationalism as counter-revolutionary ideologies is examined in terms of their effect on alliances. With respect to the future, however, nuclear weapons become an intervening element preventing ideology from serving its traditional role in shifting coalitions. In the course of providing the historical account, Dinerstein offers a number of provocative observations. Example: "The historical accounts of the 16th and 17th centuries suggest that rulers then had better information than rulers now" (p. 45).

In his chapter on international organizations, William Coplin also sees ideological conflict as less probable in the future. He identifies two international bargaining processes. In conflict bargaining (which includes ideological divisions), the other party is defined as the problem whereas in collective problem solving the difficulty is defined separately from the parties to the dispute. Coplin believes that international organizations can reduce the frequency of conflict bargaining by isolating disputes from domestic political forces that he believes push bargaining into a conflict mode. (Interestingly, Skolnikoff contends that domestic interest groups will increasingly circumvent national governments and exert pressure directly on international organizations. Thus, he implies that international organizations will become an alternative arena for conflict bargaining.) Coplin suggests that international organizations can play three progressively more active roles in international bargaining—they can provide a forum; they can play a regulative role; and they can assume a distributive role in which costs and benefits are allocated among states. During the next 20 years, he predicts, international organizations will remain a forum and play a regulative rather than a distributive role.

Although the chapters described above have considerable diversity, they share an assumption that the major future sources of change for international politics are likely to be technology and the environment rather than such matters as ideology or great power wars. In a concluding chapter on race and international relations, Tilden LeMelle and George Shepherd jolt the consensus on sources of future problems. In contrast to the other chapters, human relations become the origins of future problems. The authors propose a deterministic and inevitable sequence for racially stratified societies. At first, such societies will generate considerable cohesion or "centripetal force" through "pseudo-assimilation," and police powers. Eventually, this stage is replaced by a "centrifugal" phase in which open racial conflict develops. The authors see this second stage as prevalent in the world today. The present centrifugal stage creates such international behaviors as an increase in transnational racial ties, an increased concern with external intervention, and a tendency for existing regimes to become defensive, closed, and inwardly directed. In alternative third stages, states become uniracial, or a "pluralistic consensus" develops in which race no longer stratifies society. The authors make no predictions about the conditions leading to one or the other of these final phases.

As a collection, these essays stimulate thought about changes in international relations. The lack of explicit attention to the means for generating predictions, however, is troublesome. For example, LeMelle and Shepherd ignore the forces that move a society from the first to the second stage. Skolnikoff depends upon the citation of authorities—some of whose calculations are extremely controversial—for projecting technological and environmental developments. Dinerstein, after devoting the longest chapter in the volume to a review of historical factors, concludes with predictions based on recent technological developments. Such shortcomings may be attributable to the limited space allocated to each selection, or, alternatively, they may reflect the state of prediction about the international system.

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**Communist Penetration of the Third World.** By Edward Taborsky. (New York: Robert Speller & Sons Publishers, Inc., 1973. Pp. iii, 500. $12.50.)

Despite diligent effort, and an obviously sincere concern for his topic, Edward Taborsky, Professor of Government at the University of Texas, and a former personal aide to Eduard Benes, the late President of Czechoslovakia, has written a study of "Communist strategy" in the third world which fails to enlighten, which confuses and distracts, rather than clarifies.

For Mr. Taborsky, communism is not an objective condition created by the realities of the third world. It is not something which the third world itself chooses. It is an alien force, introduced and imposed from the outside. As a result of such assumptions, Mr. Taborsky does not focus on the way in which the third world itself *produces* a Communist response. He is concerned to tell us all the things that "communism" is doing to "exploit" the conditions of the third world, which is a "target" for "Communist" penetration.

Mr. Taborsky's difficulty is instructive, for it is typical of a kind of methodological confusion found in many studies of communism, particularly those informed by a spirit of moral hostility towards communism. Mr. Taborsky's basic flaw is not that he oversimplifies communism, or fails to appreciate the significance of polycentrism, but that he has reified the word communism. It is this reification which creates the illusion of a monolithic "communism," and the confused notion that "communism is spreading."

Mr. Taborsky, naturally enough, focuses on an analysis of Soviet strategy in the Third World, since he assumes that the Soviet Union is the leader of "world communism." Beginning with Lenin's work on imperialism, Mr. Taborsky devotes an enormous amount of space to "Marxist-Leninist" analyses of the problems of the third world produced by Soviet ideologues, on the curious assumption, apparently, that such analyses "create" communism in the third world; that is, we are to assume that the Soviet Union "exploits" these problems for the sole purpose of "spreading communism," even though Mr. Taborsky does not relate Soviet activity to the specific state interests of the Soviet Union. Even more confusing, he freely equates the activities of communist parties in the Third World with those of the Soviet state; so that the victory of Mao Tse-tung in China is viewed as a victory for something called "communism," even though Mao Tse-tung's political success cannot be shown to have benefited the Soviet Union in any way; or to have benefited any political group, for that matter, except for Mao Tse-tung's own Communist Party. In that tautological sense, of course, Mao Tse-tung's victory in China, was a victory for "communism."

In fact, Mr. Taborsky is not very much interested in the condition of the Third World, only in the methods by which "communism" "exploits" these conditions. But who are these Communists exploiting the Third World? Is it the Soviet Union? Mr. Taborsky does not consider such a question, for although he identifies the Soviet Union as the leader of "world communism," he can find evidence in support of his analysis in the activities of *any* Communists, whether or not they have any links to the Soviet Union, and he uses the same terminology of exploitation, even when the Communists he describes are themselves Third World figures, whose "exploitation" consists of their attempt to solve their own problems. In fact, in Mr. Taborsky's curious conceptual scheme, China and Cuba are no longer Third World countries, for they are now part of the "world socialist system."

Mr. Taborsky laboriously explores the differences between the "official" Communist line, which is, presumably, the position of the Soviet Union, and that of other Communist parties, in China and elsewhere. He concludes that their differences are not ideologically significant, without ever recognizing the obvious point, that the differences between the Chinese and Soviet Communist parties, for instance, reflect the fact that these parties exist to serve different interests. The Chinese Communist Party differs from the Soviet Communist Party for one overwhelming reason: it functions to serve the interests of the Chinese state, *not* the Soviet state.

Mr. Taborsky's treatment of "communism" in the third world eventually produces the kind of "twilight zone" effect frequently found in such turgid ideological studies. The book shifts back and forth from a consideration of Soviet strategy to something called "communist strategy" (pp. 36 ff, 204 ff, 447 ff.). Communist "conspirators," predictably, are endowed with superhuman powers in the manipulation of the leaders of the third world, who are too "inexperienced" and "emotional" to cope with "Communist exploitation," while their peoples "possess a highly excitable mentality oriented toward emotionalism, inclined more toward anarchism than strict discipline" (pp. 28, 98, 140) although it is not clear why these questionable attributes of Third World peoples make them more vulnerable to manipulation by people who call themselves Communists than by people who call themselves CIA agents.

Concealed within the subject matter of Mr. Taborsky's book are some very real political issues. Communism in the third world is real. The "Communist penetration" which Mr. Taborsky reads into this reality is a semantic illusion.

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**West Africa's Council of the Entente.** By Virginia Thompson. (Ithaca, N.Y.: Cornell Univeristy Press, 1972. Pp. 313. $14.50.)

Since Virginia Thompson's study was published, three of the five countries of the Entente have undergone internal upheavals: yet another Dahomean military coup unhorsed an ungainly presidential troika (itself a contrivance of an earlier military regime); the Upper Volta military retook power, sending the country's civilian government and legislature packing, and in Niger, the military also seized power from President Hamani Diori, ostensibly because his government had been unable to come to grips with the problems posed by the Sahelian drought. It would be trite—and unfortunate—to dismiss Professor Thompson's solid study because events had overtaken her account; I think it fair to argue that anyone who read her book would not be surprised at these latest developments. Herein, then, one of the more important contributions of the study: it provides not only excellent background to an understanding of recent events within the five countries, but also places these events within the broader contexts of association with each other and with their other francophone and anglophone neighbors.

The author traces the origins of the Entente to an older dispute (1956–58) about how French-speaking West Africa was to be structured after the demise of the French West African Federation, looking toward new relationships between its components and between them and France. The "federalists," championed by Senegal's Senghor (and echoed by various attempts at union involving Ghana, Guinea, and Mali), argued for a united, autonomous stand with lessened dependence on France and Europe; the Ivory Coast's Houphouet Boigny, co-author of the Loi Cadre and leader of the transterritorial RDA, formed the Entente in 1959 as a loose association that would simultaneously insure the Ivory Coast's dominance in its area *and* its close ties with metropolitan France. The author clearly indicates that the Entente has been—and remains—dominated by Houphouet; his personality, coupled with the economic and political strength of the Ivory Coast, has managed to preserve the Entente as an organization that provides minimal, but indispensable services to its members (financial assistance, brokerage for French and other foreign aid, employment for job-hungry northerners, etc.). The relatively spectacular economic success of the Ivory Coast (compared to its neighbors) has accentuated its dominance even further.

The Entente, however, has never become a true regional organization, a failing that must also be laid partly at Houphouet's door. Thompson demonstrates that the deliberately loose structures of the Entente, its inability to coordinate developmental planning and policy making, the relative weakness of the elites (low capability, lack of cohesion, personality clashes, etc.) outside of the Ivory Coast, the diffuse nature of group activity and goals, have all contributed (with Houphouet's help, to be sure) to keeping the Entente in a continuous state of semi-disarry. Houphouet was once reputed to have said that he did not want the Ivory Coast to become a "milch-cow" for its hungry neighbors; the Entente not only prevents that from happening, but puts the Ivory Coast in the happy position of being able to dispense its benevolence as political rewards. The lesson—as Professor Thompson's lucid analysis amply demonstrates—is clear for those who seek interstate association in Africa: without ample structural underpinnings and firm elite consensus, a union of equals (much less an association of unequals) has little hope of success.

Among the better chapters in the book is that focusing on "Foreign Relations and Divisive Issues," a useful compendium of recent problems and dilemmas confronting the Entente members. Relations with France are given due coverage, as are such matters as dual nationality, immigrant and migratory labor (a particular problem for the Ivory Coast, Upper Volta, Niger, and Dahomey), as well as particular issues such as relations with neighboring Ghana, the Biafran episode (the Ivory Coast was one of the few African countries friendly to Biafra) and Houphouet's ambiguous campaign to sponsor "dialogue" with South Africa.

For all the attention paid to the internal politics of the five Entente states, the book contains little information about the Entente itself as an organization—however diffuse or weak it may be. The entente remains, after all, a structure of interstate collaboration: details on that collaboration, the nature of whatever decision making takes place, the institutional arrangements that facilitate interaction, all remain somewhat vague or mysterious throughout the work. The Entente heads of state, or its ministers, do meet from time to time, and networks of interstate cooperation do function, however spasmodically. All this attests to the presence of a structure of interaction that gives form—and name—to the Entente; failure to give due attention to these matters tends to unbalance what is an otherwise admirable work.

In sum, with only the above small reservations, I commend Professor Thompson's book to as wide a readership as possible. There is little enough scholarship devoted to French-speaking Africa, and her book is a welcome addition to that literature.

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**Environmental Policy: Concepts and International Implications.** Eited by Albert E. Utton and Daniel H. Henning. (New York and London: Praeger Publishers, 1973. Pp. ix, 266. $15.00.)

The trouble with collected articles on environmental politics has been that they were usually fat with descriptions of environmental problems but lean on the discussion of political aspects. This book concentrates resolutely on the political aspects of environmental degradation and thereby earns some distinction among a generally dismal collection of collections on ecological politics. The major problem is that these twenty-six short, but generally useful, studies on many facets of environmental politics have almost been made to appear a cheerless potpourri of scholarly trivia by utterly negligent editing—*almost*. If the reader will sample some articles, perhaps using the topical guide I shall suggest, he will find the material usually interesting and often stimulating if seldom exciting. It is a good collection of reference articles for scholars, classroom teachers, and students, one that raises a variety of relevant issues on many aspects of environmental politics.

This collection of articles from *Natural Resources Journal* (July, 1971, and April, 1972), each customarily running between 10 and 12 pages, covers (by my logic) five broad topics: international aspects of environmental politics (ten articles), general political implications (five articles), environmental law and litigation (four articles), the public and environmental politics (three articles), and problems of governmental policy-making (four articles). A number of the writers, including Grant McConnell, Lynton K. Caldwell, and Richard Falk will be familiar to scholars in public policy, while others such as Charles R. Ross (formerly a member of the Federal Power Commission) and Dean Arnold W. Bolle (Chairman of the University of Montana committee whose report on the Bitterroot National Forest precipitated a national battle over clear-cutting) had important practical experience in environmental policy making. Substantively, the most useful material for scholars is likely to be (1) the articles on international environmental control—an important topic about which scant research presently exists—and (2) the studies of legislative, administrative, and judicial policy implementation. On implementation, the articles by David Sive on power-plant siting, L. F. E. Goldie on pollution from deep-sea mining, and Ian Brownlie's on standard setting in general are instructive not only because they provide an operational understanding of the technical and political problems associated with policy implementation but because they are a healthy antidote to the sometimes unworkable political prescriptions urged by overzealous environmental reformers—a lesson in practical politics (and applied science) that's just the thing for students to read, for instance, before setting out to reform the system.

The international environmental articles, the largest and most varied group in the collection, are especially welcome because they discuss a number of specific matters including the protection of migratory animals, the special problems of environmental regulations in Africa, and administrative designs for international regulatory agencies, albeit more briefly than one would wish. Perhaps the least interesting material relates to environmental litigation, which has been extensively investigated by a number of experts in far more exhaustive studies, and the treatment of the general political problems associated with environmental protection; most of the latter is either a distillation of earlier, better studies by the authors, or (in the case of Senator Henry Jackson's opening piece) mainly exhortatory. In general, however, most of the material merits at least some study.

The most pervasive problem with the articles themselves is that they are too brief; they are often suggestive in outlining general concepts and problems, or in giving the reader a few selected insights into topics, but they seldom carry him into much depth. The material would serve best as a stimulating introduction to selected topics for laymen, students, and scholars, as a goad to the development of research topics and collection of empirical material, as a reference book suitable for library purchase or for the collection of a scholar with a continuing interest in environmental politics. Also, the material uniformly illustrates one of the major current problems in research on environmental policy: the almost total absence of carefully integrated empirical approaches to the major research problems associated with environmental politics. Except for Matthew Crenson's fine *The Un-Politics of Air Pollution* (1971) and a scattering of other studies, such material combining conceptual and empirical approaches is regrettably rare.

As for the "editing," these authors—any authors—deserve better. There are no introductions to the material, no topical sections into which articles can be organized for the reader's convenience, no attempts to inform the reader of the authors' often considerable authority or distinction on their chosen areas (as in the case of Charles R. Ross and Dean Arnold W. Bolle), no effort by editors or publishers to make a potentially useful set of studies actually useful. Well, the editors were not wholly negligent: they did include their own biographic entries while omitting those of the authors. It's that kind of job.

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**Strategic Energy Supply and National Security.** By Carl Vansant. (New York: Praeger Publishers, 1971. Pp. 150. $15.00.)

If you really want to know that the Navy bought 11,375,000 barrels of JP-5 jet fuel in 1963 and the Army 898,000 barrels of Arctic diesel fuel, and are willing to pay $15 for that and a lot of similar information, this may be just the book for you. But if you are interested in the somewhat broader questions implied in the title of the book you had best turn elsewhere.

One of the starting points in looking at such questions is a comparison of military demands for petroleum products with U.S. total consumption. Vansant, an operations analyst in private practice, doesn't bother to make that simple calculation, and even makes it unnecessarily difficult for his readers. Like many others who write on energy, he uses inconsistent units, giving figures for national consumption in metric tons per year and for military consumption in barrels. He then goes the last mile in further confusing things by providing, without explanation, disparate figures for military deliveries (or procurements): 416, 339, and 393 million barrels (in Tables 15, 16, and 17 respectively), all for 1968. Using the intermediate value and making appropriate conversions, one can calculate that military consumption of petroleum products in 1968, a war year, was about 10 per cent of civil consumption. With U.S. imports at about 20 per cent of total consumption at the time, it would seem that the nation might make do with belt tightening in an emergency. One is led, then to a further series of questions. Will the situation change significantly during the next few years? How much would military demand drop during peacetime, or increase in a "hotter" war? How long could the military operate on reserves? What fraction of civil demand would be essential to effective conduct of a war? Are there critical products that the military might require which would pose special problems if foreign oil were unavailable? Alas, Vansant doesn't address such questions. Nor does he treat such issues as the impact of increased oil prices on the economy and political stability of oil-importing countries, presently a critical security issue in the case of Italy and India and potentially one for many other states as well.

Next, there are the omissions. There is nary a word in the book about whether Alaskan north-slope oil will make a difference in the future U.S. oil position or North Sea oil in that of the U.K. Oil shale and tar sands are merely mentioned, with no estimates of amounts of oil that might be obtained from such sources or of cost factors; and geothermal power is not treated at all. Generally, single estimates are given for fuel reserves, when in fact the estimates are critically dependent on assumptions about what costs of exploitation are acceptable, and otherwise vary enormously.

Third, the book is not without errors of fact. Did you know, for example, that the population of Israel is half the population of the United Arab Republic (p. 9)?

Finally, there are a host of other problems, some, but not all of which, may have their antecedents in the book's obsolescence. Published in 1971, it is now hopelessly out of date. The author observes (p. 4) that "As far as oil is concerned, the world is clearly divided down the middle—East and West." This is clearly not true, and it is doubtful that it could have been reasonably so described three years ago. As a minimum, one would do better to divide it into four groups: the western-industrial importing states, the OPEC nations, the oil-importing developing nations, and the Soviet bloc.

On page 11, Vansant observes "The Russians got a fantastic bargain for their money [in Egypt]." That might have been a reasonable judgment in 1971 although there were signs as early as May of that year that Sadat would resist Soviet domination. Certainly few would now argue that the Russians have gotten a good return on their investment.

When I agreed to review Vansant's book I did so because I thought, and still do think, that the resources issue may be displacing ideology as the major source of instability and conflict in international relations. I had hoped to read something that was informative and thought-provoking, but these hopes were disappointed.

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**Russia and Asia: Essays on the Influence of Russia on the Asian Peoples.** Edited by Wayne S. Vucinich. (Stanford, Calif.: Hoover Institution Press, 1972. Pp. 521. $15.00.)

This is a most useful volume offering a broad review of Russia's role in Asia, even though it concentrates primarily on the impact of this role on the Asians now within the Soviet boundaries and on the Russians' perception of this role. A treatment of most of the neighbors of the USSR (only two are included here) must await another volume. As in most symposia, contributions are not always even in quality, but the central theme is well developed. An impressive wealth of data, in the text as well as in the 96 pages of exceptionally comprehensive footnotes, is a major strength of the book. It is an invaluable source of factual information on a little-known subject and frequently covers virgin territory. Its major weakness, I believe, is its relative absence of analysis (with few notable exceptions such as chapters by Mark Mancall and Alexandre Bennigsen) and its relative concentration on the prerevolutionary and early Soviet periods. Only four chapters (by Vucinich, Bennigsen, Lang, and the Dunns), treat the post-Stalinist period in depth. This is unfortunate because, as shown by recent scholarship, it is only in this period that the impact on Soviet Asians of Russian culture, modernization, and sovietization began to emerge into a new synthesis. Thematically the book has three parts: three chapters are devoted to the Russian view of Asia and to Russian and Soviet orientology; five chapters review Soviet Asian groups; two chapters discuss Russia's impact on the two most important of its Far Eastern neighbors: China and Japan.

In the first group Nicholas V. Riasanovsky contrasts the ancient root hatred of Asian invaders by the Russian masses with Europe-inspired imperialism and sense of *mission civilisatrice* of the post-Petrine educated classes, and with the fascination on the part of groups of creative intelligentsia at the turn of the century with the new integrative Eurasian concept. In the follow-up discussion of Russian Oriental Studies, Richard N. Frye, a noted Iranian rather than Russian scholar, offers the controversial views that Mongol invasion neither cut Russia off from the West nor redirected its destiny eastward (pp. 31–32) and that the future of Oriental studies in the USSR is "in the hands of classical scholars or academics" (p. 51). No refutation of the latter view is needed here as the reader then progresses to the next chapter (Soviet orientology) by the volume's editor, in which the political nature of governmental sponsorship of Soviet orientology is exhaustively documented, without detracting from the quality of its scholarship. Truly encyclopedic, Vucinich's chapter escapes being a catalogue of institutions, resources, scholars and bibliographies of Soviet orientalia, because of the author's knowledge of historical, political and cultural context. The five specific chapters, all informative and well-documented, are uneven in analytical emphasis and in period coverage. They treat Moslems of European Russia and the Caucasus (Alexandre Bennigsen), Armenia (Vartan Gregorian), Georgia (David M. Lang), Central Asia (Manuel Sarkisyanz) and Peoples of Siberia and the Far East (Stephen P. and Ethel Dunn). One might wish to have a more differentiated and more recent coverage of the five major groups of Central Asia. Had the post-1956 period been included, perhaps the statement that "no original Uzbek thought, literary or political, has shown itself since the Time of the Purges" (p. 284) would have been omitted. One also finds Lang's characterization of Soviet policies in Georgia as non-integrationist (p. 245), somewhat optimistic in view of the 1973 fall of Mzhavanadze, "a very fine patriot and gentleman" (p. 239).

The last part of the book gives a comprehensive survey of Russo-Japanese relations by George Alexander Lensen, and Mark Mancall's analysis of "The Structure of Contact" between Russia and China. Contrasting the discrepancy between the nonessential nature and volume of Russo-Chinese political and economic contacts and the far-reaching impact of Russian influence on the modern Chinese political culture, Mancall advances a theory that the change in the Chinese worldview took place because of the preoccupation of Chinese intellectuals with the classical Russian literature and their identification with oppression described. (The impact also came with ideological communist writings, many transmitted by the Russians, if not of Russian origin, pp. 329–331). It is interesting that Lenson makes the first point also, in application to the Japanese intellectuals (pp. 364–366). Mancall argues that the Russian concept of synchronic conflict explaining social conflict and discontinuity, hitherto unknown to the Chinese paradigm of universe of harmony and endless repetitiveness, has added new dimensions to this paradigm, with profound results for the Chinese society (pp. 332–336).

In summary, the book adds to the still little known area of the impact of Russians in contiguous Asia and is an important source of instruction for interested laymen and scholars.

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**The Austrian Example.** By Kurt Waldheim. Translated by Ewald Osers. (New York: Macmillan, 1973. Pp. 230. $6.95.)

Much the greater part of this book was published as *Der Österreichische Weg* in 1971 when its author, having failed to become federal president of Austria, was a prominent candidate for the succession to U Thant. It now appears in English with short chapters added at each end which reflect the successful candidate's new status in a studied inoffensiveness, a style appropriate to the international character of his office as Secretary-General of the United Nations. The rest of the book is hardly less bland. Waldheim's record of careful and correct behavior, his diplomatic professionalism in fact, must have been one of the main reasons for his acceptability, and if his writing is singularly uncontroversial this is just the other side of the coin.

Successive chapters deal with Austrian history since 1918 and aspects of foreign policy under the Second Republic, the main lines of which Waldheim endorses with something like enthusiasm. On neutrality, Waldheim is at pains to emphasize the policy's status in Austrian parliamentary legislation, and its domestic origins in the thinking of Karl Renner and other Austrian statesmen. He is anxious, perhaps unnecessarily, lest it be thought that neutralization was simply imposed on a passive Austria by fiat of the Powers. Although he was actively involved in Austria's diplomatic efforts to secure full independence, Allied military withdrawal, and the international treaty of 1955, his account scrupulously restricts itself to secondary sources, the published memoirs of diplomats, politicians, and historians. He has very little to say—or, perhaps, he has chosen to say little here—on Austria's relations with the Powers, with Eastern European states or with Yugoslavia. Even the chapter on economic aid to less-developed countries is only in part an account of Austrian experience and largely a rehearsal of familiar facts and figures.

The most interesting chapters are those concerned with two of the outstanding problems which faced Austria when Waldheim was Foreign Minister (1968–70): the South Tyrol dispute and relations with the European Economic Community.

Waldheim's account of the South Tyrol dispute is the most closely detailed part of the book and sheds light particularly on the later phases of the "timetable" settlement negotiations, conducted in London, Paris and (finally) Copenhagen. He concludes, in what we may take to be the quintessence of his general approach to international problems: "On the South Tyrol issue, as on any other issue, it was ultimately necessary to think in terms of realistic politics and not to indulge in illusions that might have had disastrous consequences for the minority concerned."

He goes on immediately (p. 150) to reaffirm that Austria's "protective rôle" has not expired: on the contrary, the Gruber-De Gasperi Agreement of 5 September 1946 "is still in force." What emerges from this chapter and from the one on European integration is an understandable resentment of erstwhile Italian efforts to link the erection of barriers against Austrian participation in the EEC to the South Tyrol dispute. Waldheim and his predecessors already had their share of difficulties arising from Soviet and (at times) French opposition to a closer relationship between Austria and the Community.

Unfortunately, the book suffers from indifferent translation and production. The Foreign Minister of Sweden from 1945 to 1962 was Östen Undén, not Lunden (p. 104). The Pearson Report of 1969 had to do not with *Pattern* but with *Partners in Development* (pp. 211–12). As ill luck would have it, the brief chapter on Austria in the UN is the one most thickly studded with inaccuracies, to the embarrassment, no doubt, of the unwitting author who appears not to have checked the translation. There is, regrettably, no "Committee for the Protection of Minorities" (p. 160) in the UN system, although there is a *subcommission* which brackets the protection of minorities with the prevention of discrimination. The "second United Nations decade" (p. 164) turns out to be the Second Development Decade. The reference to "the space agency" (p. 159) suggests a specialized agency; but there isn't one, so from the context one concludes that a reference must be intended to Ambassador Peter Jankowitsch's chairmanship of the UN Committee on the Peaceful Uses of Outer Space. "The first international state conference" (p. 159), however, had me defeated; as did the typesetting on p. 160 where two successive paragraphs totter off the page into incomprehensibility.

This is all the sadder because the subject is of the utmost interest. Austria having long since achieved the reputation of a "good" UN member, it is disappointing that Waldheim, who headed the Austrian mission to the UN for so many years (1955–56, 1960–64, 1970–71), modestly refrains from telling us how this was done. Perhaps there is a clue to Austria's success early in the book (p. 8), where Waldheim writes: "Today Austria's diplomats represent a small state . . . Since [small states] are deprived of any real power, there is all the more reason for them to demonstrate their usefulness to other nations through an intelligent, well thought-out and often conciliatory foreign policy, thereby also serving their own interests."

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**Documents on International Affairs, 1963.** Edited by D. C. Watt, James Mayall, and Cornelia Navari. (New York: Oxford University Press, 1973. Pp. xiii, 517. $38.50.)

With this volume the Royal Institute of International Affairs (Chatham House) has issued its final collection of documents in a series which began with volumes on world affairs in the 1920s. The series contained two components: the *Survey of International Affairs,* which analyzed world events and *Documents on International Affairs,* which presented documentation on those events.

The final *Survey* for 1963 has yet to be published. This makes a complete evaluation of the book under review difficult, for it is designed in part as a documentary supplement, with the material arranged to correspond to the chapters in the *Survey*. Perhaps because of this two-part format there are no editorial introductions to the some 150 documents in this volume, a serious deficiency in light of the great range of issues dealt with.

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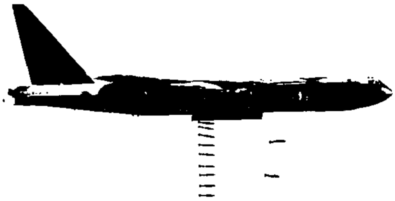

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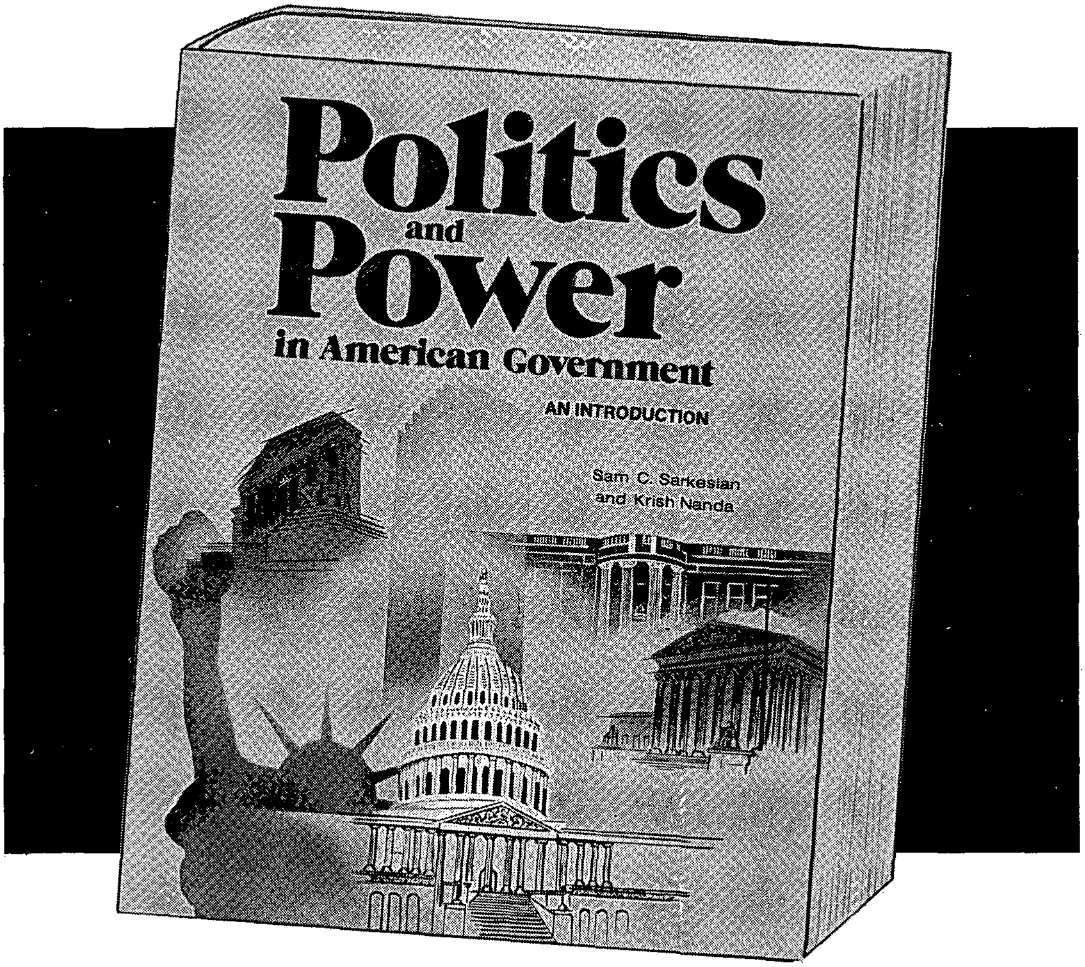
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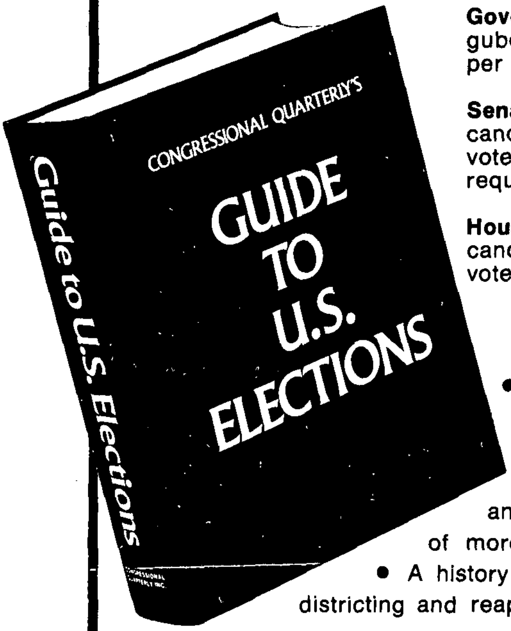

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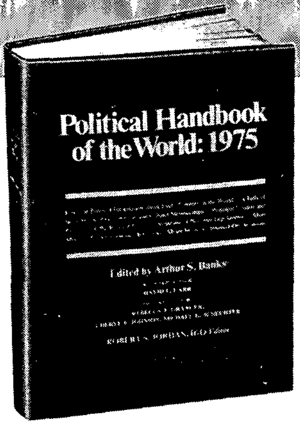
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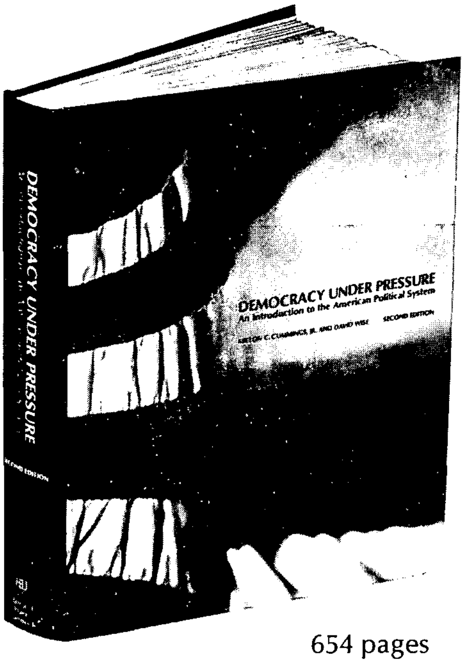

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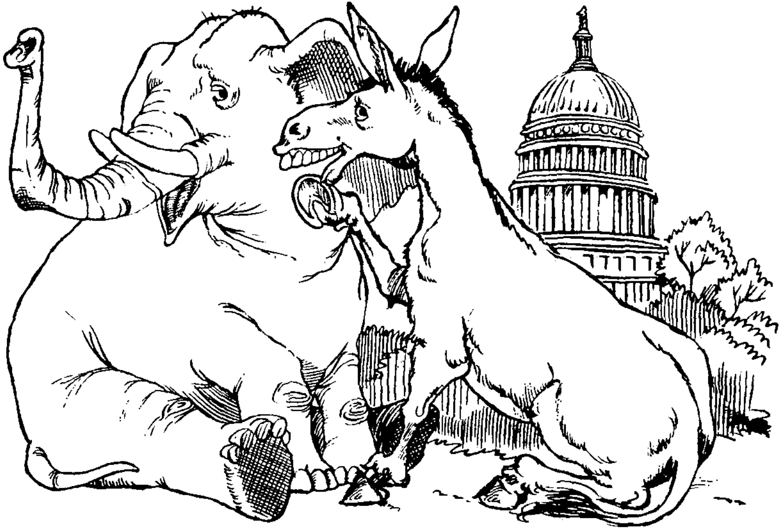

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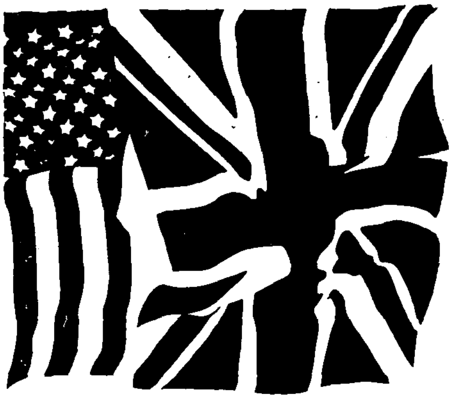

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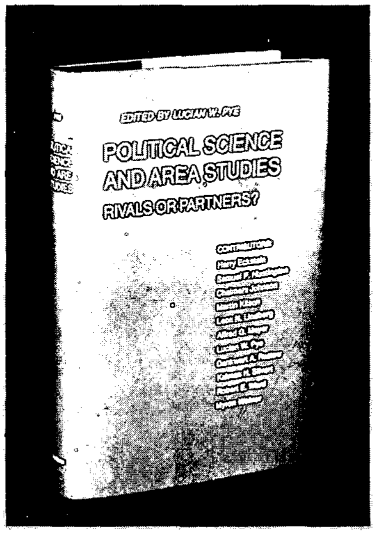

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